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JULY, 1837.

Practical and Theoretical Mechanics.

Report of the Committee of the Franklin Institute of the State of Pennsylvania on the Explosions of Steam Boilers, of Experiments made at the request of the Treasury Department of the United States. PART II. *Containing the report of the sub-committee to whom was referred the examination of the strength of the materials employed in the construction of Steam Boilers.*

[CONTINUED FROM PAGE 451.]

Strength of iron made from different sorts of Pig-metal.

The experiments to determine the effect of different kinds of pig-metal, either separate or in mixture, when converted into wrought iron, by the same refining process, were performed on bars furnished by the Salisbury Iron Company, of Salisbury, Connecticut, of which one specimen, from which were formed the two bars 218 A., 218 B., was produced from *dead gray pig;* 219 A., and 219 B., were from a specimen formed from *lively gray pig;* 220 A. and 220 B., from *mottled pig;* 221 A. and 221 B. from *white pig;* and 222 A. and 222 B., from a mixture of all these kinds together. By a reference to the tables (from LXXX. to LXXXVIII. inclusive) containing details of the trials upon these bars, it will be seen that on all of them, some experiments were made at high temperatures, and of course that the purpose of the present comparison can be properly accomplished, only by referring to those, which were made at, or near the same temperature. The experiments at ordinary temperatures embracing the mean strength, as well as the irregularities of structure, are preferred as most satisfactory in referrence to this point. In presenting these results, care has

been taken to exclude all those trials in which the effect of heat would be appreciable, either during or subsequent to the time of trial.

It will be seen that if we take into view all the bars of this iron, on which experiments were made after they were reduced to a uniform size, and exclude only 219 B., on which the sections were all deeply filed, the advantage will appear to be in favour of the metal manufactured from *white* pig ; next to which, is that produced from *lively gray*, giving 98½ per cent. of the strength of the first. Next, in the order of strength, will be found the iron from *dead gray pig*, inferior to the first by 1 2-3 per cent.; next, that from the mixture of the four kinds of pig which appears to have been weaker than the same by 4 4-10 per cent.; and, finally that from mottled pig in which the inferiority extended to 5 per cent. The following table (LXXIX.) exhibits, at a view, the comparative strength, and the respective degrees of uniformity of the several bars, with the strength of some of them at high temperatures.

At elevated temperatures, the results, except that on No. 219 B, are much nearer to each other, than those at the points selected for our general comparison. On that bar, the trials were upon filed sections. The experiment at 573°, giving a strength of 66620, exceeded those at corresponding temperatures on the other bars, by an average of about 6222 lbs., or 10⅓ per cent.; while the two experiments which were made upon it at low temperatures, as will be seen by table LXXXII., gave results, the mean of which being 66724 lbs., surpasses that of the other nine bars by 9275 lbs., or by 16 1-10 per cent. Hence we are compelled to believe that this specimen, as it came to hand, had undergone the process of hammer-hardening,—a process which the direct experiments of the committee have proved to be capable of essenially modifying the tenacity of the metal.

From the above, it appears, that the greatest difference of strength which under ordinary circumstances, can be attributed to differences in the pig-metal* from which wrought iron is produced, is about 5 per cent., and that under every mode of trial, the article formed from a mixture of different kinds of pig, is inferior in tenacity and uniformity to those derived from either of the ingredients, unless we except that from *mottled gray*. And even this latter will, on a comparison of all the experiments made upon it, under every circumstance, be found superior to the bars from *mixed* castings.

If we take into the amount 219 B., the order of values, beginning with the highest, will be *lively gray*, *white*, *dead gray*, *mixed pigs*, *mottled;* and if we arrange them in the order of their values, as deduced from a comparison of *all the experiments*, on each kind of iron, with the number of trials made on each, we have:—1. *lively gray* 15 experiments.—2. *white* 27 experiments.—3. *mottled gray* 36 experiments.—4. *dead gray* 21 experiments.—5. *mixed metals* 31 experiments.

So far as these experiments may be considered decisive of the question, they favour the lighter complexion of the cast metal, in preference to the darker and mottled varieties, and they place the mixture of different sorts, among the worst modifications of the materials to be used, where the object is mere tenacity.

* It will be understood that this remark does not apply to pig-iron contaminated with sulphur, phosphorus, copper, or other similar impurities; but only to such as contain different proportions of the *ordinary* ingredients.

TABLE LXXIX.

Comparative table of the effects of using different sorts of pig metal.

No. of the bar.	No. of the expt's. on each bar, made at the comparable temp.	Strength exhibited at each experiment.	Mean Strength of each bar.	Difference of maximum and minimum of each bar.	Amount of irregularity in parts of the mean strength.	Name of the pig, and mean strength of the two bars of each kind.	REMARKS.
218 A.	1	58683				Dead gray pig.	This bar, at 554°, possessed a strength of 60457 lbs. per square inch.
	2	57565					
	10	58459					
	11	61225	59155	4360	.073		
218 B.	1	53314				58016	
	2	55575					
	3	56339					
	4	58682					
	5	60478	56877	7164	.126		
219 A.	1	52257				Lively gray.	This bar at 630° broke with 60010 lbs. per. sq. in.
	2	59418					
	3	60204					
	4	57854					
	8	60645	58075	8388	.144		
219 B.	2	65640				62399	These two experiments were made on very deeply filed sections. At 573°, this bar gave 66620.
	3	67808	66724	2168	.032		
220 A.	1	52503				Mottled.	At 575° broke with 60988 lbs. per sq. inch.
	2	52119					
	3	53999	52873	1496	.028		
220 B.	1	58642				56062	
	2	58108					
	3	58125					
	10	62138	59252	4027	.068		
221 A.	1	53021				White.	At 520° broke under a strain of 60322 lbs. per. sq. inch.
	8	62862					
	9	63081	59654	10060	.168		
221 B.	1	54764				58986	
	2	56054					
	11	57901					
	12	61736					
	13	61140	58319	6972	.119		
222 A.	1	49597				Mixed pigs.	
	2	58924					
	3	60651					
	4	61915					
	5	58895	57996	12318	.212		
222 B.	1	51132				56419	This bar gave as the mean of two experiments at 572° 60215 lbs.
	2	53703					
	3	54407					
	4	59652	54843	8500	.155		

TABLE LXXX.

Experiments on bar 218 A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut, the ore being obtained from "Ore Hill," in that town. The kind of pig used, the "dead gray." Refined after the English method; a loup made and a "gun bar" formed

Marks.	Breadth.	Thickness.	Area at the sections measured before trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature Fahrenheit.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.	Effective strain.
0	.771	.231	.178101		1833.						
1	.755	.231	.174405	1	April 20,	.172895	55° 6	356	10680	534	10146
2	.755	.230	.173650								
3	.755	.229	.172895	2	"	.176253	71	356	10680	534	10146
4	.755	.230	.173650								
5	.755	.230	.173650	3	April 25,	.172666	554	366	10980	549	10431
6	.754	.230	.173420								
7	.756	.230	.173880	4	"	.173420	71	397	11910	595	11315
8	.755	.230	.173650								
9	.753	.230	.173190	5	"	.173420	71	397	11910	595	11315
10	.753	.230	.173190								
11	.754	.229	.172666	6	"	.173765	68	388	11640	582	11058
12	.754	.229	.172666								
13	.754	.229	.172666	7	"	.173470	68	396	11880	594	11286
14	.754	.230	.173420								
15	.754	.230	.173420	8	"	.173190	68	404	12120	606	11514
16	.754	.230	.173420								
17	.754	.230	.173420	9	"	.173497	68	392	11760	588	11172
18	.755	.227	.171385		Mn. of 9 =	.173611					
Mean of 19			.173512								
Maximum			.173101								
Minimum			.171385								
Mn. of these 2			.174743								
Diff. of the 2			.001716								

Experiments on the specimen as received, filing successively two different sections,

Br.	Th.									
.486	.361	10	April 22,	.153576	50	315	9450	472	8978
.502	.441	11	"	.221382	68.5	481	14430	721	13709

TABLE LXXX.

expressly for the purpose of affording a specimen for these experiments. The specimen received was drawn out into two bars, A and B, under the hammer, previously to which, however, two sections were filed on the edges and a fracture made at each. Specific gravity 7.7397.

Strength in pounds per square inch.	Point of fracture.	REMARKS.
58683	No. 3	Part in tin from $11\frac{1}{2}$ to $15\frac{1}{2}$. Fracture near the wedges.
57565	" $0\frac{1}{2}$	
60412	" $12\frac{3}{4}$	Broke in tin.
65246	" $14\frac{7}{8}$	Had been in tin at this point.
65246	" $16\frac{3}{4}$	
63638	" $6\frac{3}{4}$	
65079	" $8\frac{1}{2}$	
66482	" $9\frac{3}{4}$	Near the tinned part.
64392	" $5\frac{3}{8}$	Had not been in or near the tin. The mean area of the 9 sections of fracture is .000099 square inch greater than that of the 19 sections measured before trial.

and breaking it at each.

58459		This section was filed very deep before the bar had been reduced to uniform size.
61925		Filing much less than in the preceding section.

TABLE LXXXI.

Experiments on bar No. 218 *B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "ore hill" in that town. The kind of metal used, "dead gray pig." Refined after the English method.*

Marks.	Breadth.	Thickness.	Area before fracture.	No. of the exp't.	DATE.	Area of the section of fracture.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.
0	.763	.227	.173206		1833.					
1	.761	.227	.172747	1	April 27,	.174269	580°	326	9780	489
2	.761	.226	.171986							
3	.761	.227	.172747							
4	.761	.227	.172747	2	"	.174879	96	341	10230	511
5	.761	.227	.172747							
6	.761	.229	.174269							
7	.761	.230	.175030	3	"	.175030	90	346	10380	519
8	.761	.230	.175030							
9	.761	.230	.175030							
10	.761	.230	.175030	4	"	.174840	74.5	360	10800	540
11	.761	.230	.175030							
12	.761	.229	.174269							
13	.761	.230	.175030	5	"	.174840	74	371	11130	556
14	.761	.230	.175030							
15	.762	.229	.174498							
16	.762	.230	.175260	6	May 2,	.175030	77	400	12000	600
17	.761	.229	.174269							
18	.761	.229	.174269							
18.3	.762	.230	.175260	7	"	.172747	77	400	12000	600
		Mean of 20	.174173	8	"	.172747	77	400	12000	600
		Maximum	.175260							
		Minimum	.171986	9	"	.172747	77	398	11940	597
		Mean of the 2	.173623							
		Diff. of the 2	.003274	10	"	.175030	77	398	11940	597
					Mean of 10	.174186				

TABLE LXXXI.

A loup made, and a gun bar formed expressly for the purpose of affording a specimen for these experiments. Drawn under the hammer, reduced by filing, and gauged at every inch from 0 *to* 18 3–10 *inclusive. Specific gravity*, 7.7397.

Effective strain.	Strength in lbs. per square inch.	Point of fracture.	REMARKS.
9291	53314	No. 17½	In tin from 3 to 6½.
9719	55575	" 15½	
9861	56339	" 14	
10260	58682	" 12¾	
10574	60478	" 11¼	
11400	65132	" 10	
11400	65992	" 5	Broke without additional weight.
11400	65992	" 3½	Had been in tin.
11343	65662	" 1	
11343	64806	" 8½	Mean area of 10 sections of fracture is *greater* than that of 20 measured sections by .000013.

TABLE LXXXII.

Experiments on bars No. 219 *A and* 219 *B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut, the ore obtained from "Ore Hill," in that town. The kind of pig used,* lively gray. *Refined after the English method. A loup made and a gun*

Marks. *Bar No.* 219 *A.*	Breadth.	Thickness.	Area of the measured sections before trial.	Marks.	Breadth.	Thickness.	Area after trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight X leverage.
0	.754	.200	.150800										
1	.757	.197	.149129										
2	.759	.198	.150282						1833.				
3	.757	.199	.150643	Means after the 9th exper't.				1	Jan. 26,	.145626	63°	267	8010
4	.758	.199	.150842										
5	.756	.196	.148176	5	.713	.168	.119784						
6	.754	.196	.147784	6	.713	.187	.133331						
7	.755	.197	.148735	7	.717	.189	.135513	2	"	.147227	50.5	307	9210
8	.755	.197	.148735	8	.722	.190	.137180	3	"	.146286	55.5	309	9270
9	.755	.197	.148735	9	.726	.186	.135036	4	Feb. 2,	.150742	596	306	9180
10	.755	.196	.147980	10	.723	.188	.135924	5	"	.148176	630	312	9360
11	.752	.197	.148144	11	.714	.188	.135232	6	"	.147236	59	324	9720
12	.753	.197	.148341	12	.711	.186	.132246	7	"	.147420	60	341	10230
13	.755	.198	.149490	13	.712	.183	.130296	8	"	.150382	60	320	9600
14	.755	.197	.148735	14	.701	.180	.126180						
15	.756	.196	.148176					9	Feb. 7,	.148176	574	335	10050
16	.756	.195	.147420	16	.670	.175	.117250						
17	.758	.196	.148568	17	.715	.185	.132275	10	"	.149238	54	351	10530
18	.757	.197	.149129	18	.720	.188	.135360						
19	.757	.196	.148372	19	.721	.187	.134827	11	"	.148242	54	355	10650
20	.757	.193	.146101										
21	.756	.192	.145152					12	"	.148735	60	361	10830
22	.756	.195	.147420										
23	.757	.196	.148372					13	"	.148016	65	367	11010
24	.755	.198	.149490										
25	.757	.197	.149129										
26	.754	.194	.146276										
26.4	.753	.196	.147588										
Mean of 28			.148478										
					Breadth.	Thick.							
Maximum			.150842		.746	.199		1	"	.148454	572	347	10410
Minimum			.145154		.751	.196		2	"	.147196	65.25	339	10170
					.729	.192		3	"	.139968	65	333	9990
Mn. of the 2			.147998										
Diff. of the 2			.005688										

TABLE LXXXII.

bar formed for these experiments. Drawn under the hammer, and bar A. reduced to a uniform size by filing. Bar B. filed deeply at three several points and broken at each filing. Specific gravity 7.8004.

Friction.	Effective strain.	Strength in lbs. per square inch.	Weight producing extension.	Extension measured before the first fracture while the bar was relieved from strain.	Point fractured.	REMARKS.
			112	1st percept.		
			203	.50 in 24		
			215	.78 "		
			225	1.00 "	No.	
400	7610	52257	235	1.27 "	20½	Broke at the smallest section in the bar.
			244	1.60 "		
			252	1.89 "		
			260	2.44 "		
			267	2.81 Broke.		
460	8750	59418			25⅔	
463	8807	60204			21½	
459	8721	57854			3½	Part in tin from 13½ to 17.
468	8892	60010			15	Broke in tin at the hottest part.
486	9234	62036			19½	
511	9719	65903			16	
480	9120	60645			0¼	
502	9548	64437			5	Broke near the wedges, part in tin from 7½ to 10½.
526	10004	67033			13⅓	Fracture farthest point from the part heated in the last experiment.
532	10118	68253			11½	Part last in tin not yet broken.
541	10289	69175			7⅓	This part was in the packing at the last experiment in hot metal.
550	10460	70668			9⅝	Had been in tin at 574°. The mean area of the 13 sections of fracture is .000074 square inch *less* than that of the 28 measured sections.
520	9890	66620				Broke in tin.
508	9662	65640				Cold fracture.
499	9491	67808				Do.

TABLE LXXXIII.

Experiments on bar No. 220 A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "Ore Hill," in that town. The kind of pig used, mottled. *Refined after the English method. A loup made, and a gun bar formed*

Marks.	Breadth.	Thickness.	Area before trial.	Marks.	Breadth.	Thickness.	Area after trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.
0	.762	.215	.163830	Measured after exp't. 9th.					1833.			
1	.762	.215	.163830	10	.702	.194	.136188	1	Feb. 23,	.163400	69.5°	301
2	.762	.216	.164592	11	705	.197	.138885					
3	.763	.215	.164045	12	.710	.198	.140580	2	"	.164045	49.5	300
4	.763	.216	.164808	13	.707	.198	.139986					
5	.763	.215	.164045	14	.705	.196	.138180	3	"	.163615	49.5	310
6	.762	.215	.163830	15	.709	.199	.141091					
7	.762	.215	.163830	16	.711	.199	.141489	4	"	.163400	620.	335
8	.760	.215	.163400	17	.716	.200	.143200					
9	.760	.215	.163400	18	.704	.197	.138688	5	"	.163830	650.	363
10	.760	.216	.164160	19	.720	.200	.144000					
11	.762	.216	.164592	20	.721	.200	.144200	6	Feb. 28,	.163830	60.	384
12	.759	.215	.163185	21	.718	.199	.142882					
13	.758	.216	.163728	22	.713	.198	.141174	7	"	.164426	55.	386
14	.759	.216	.163944	23	.706	.196	.138376					
15	.760	.215	.163400	24	.708	.197	.139476	8	"	.164299	55.	386
16	.763	.215	.164045	25	.703	.195	.137085					
17	.762	.214	.163068									
18	.761	.214	.162854	After experiment 12.				9	"	.164211	55.	391
19	.762	.214	.163068	12	.701	.199	.139499					
20	.762	.215	.163830	13	.700	.198	.138600					
21	.762	.215	.163830	14	.696	.197	.137112	10	"	.164448	580.	344
22	.762	.215	.163830	15	.693	.194	.134442					
23	.761	.215	.163615					11	"	.163507	580.	345
24	.760	.215	.163400									
25	.761	.215	.163615	18	.673	.190	.127870	12	"	.163556	575.	350
26	.760	.215	.163400	19	.708	.200	.141600					
27	.762	.215	.163830	20	.710	.198	.140580	13	"	.163780	56.5	354
28	.763	.215	.164045	21	.710	.197	.139870					
29	.763	.215	.164045	22	.704	.201	.141504	14	"	.164376	57.5	354
29.6	.760	.215	.163400	23	.698	.193	.134714	15	"	.163321	58.5	372
								16	Mar. 2,	.163728	59.5	372
	Mean of 31		.163755					17	"	.163782	59.5	372
								18	"	.163400	60.5	368
	Maximum		.164808					19	"	.163776	60.5	368
	Minimum		.162854					20	"	.163322	61.	381
								21	"	.162907	61.	381
	Mn. of these 2		.163831						Mean of 21	.163755		
	Diff. of the 2		.001954									

TABLE LXXXIII.

expressly for these experiments. Drawn under the hammer, reduced by filing, and gauged at every inch from 0 *to* 26.6, *inclusive. Specific gravity,* 7.7855.

Weight × leverage.	Friction.	Effective strain.	Strength in pounds per square inch.	Point of fracture.	REMARKS.
9030	451	8579	52503	No. 9	
9000	450	8550	52119	" 29	A part not included in the first experiment, the bar being too long to be taken all at once into the machine.
9300	465	8835	53999	" 26½	
10050	502	9548	58433	" 8	Part in tin from 3½ to 6. After this experiment, sent the bar forward and took hold again, without changing the tin.
10890	544	10346	63151	" 7	Broke at the coldest part.
11520	576	10944	66801	" 6	
11580	579	11001	66905	" 4½	
11580	579	11001	66958	" 3⅛	
11730	586	11144	67864	" 1½	This section of the bar is now finished. Part from 9 to 26½ was then gauged again. This section had not been in tin.
10320	516	9804	59618	" 10⅜	Broke near the wedges. Part in tin from 15 to 18½.
10350	517	9833	60138	" 24½	Broke near the wedges.
10500	525	9975	60988	" 16½	Broke *in the tin*.
10620	531	10089	61601	" 9½	Within the gripe of the wedges.
10620	531	10089	61378	" 10½	Do. do.
11160	558	10602	64915	" 12½	
11160	558	10602	64754	" 13	
11160	558	10602	64610	" 13⅜	Near the wedges.
11040	552	10488	64186	" 24	Remote from tinned part.
11040	552	10488	64039	" 22½	
11430	571	10859	66488	" 19½	
11430	571	10859	66658	" 18½	
					The mean area of the 21 sections of fracture is *identical* with that of the 31 measured sections.

TABLE LXXXIV.

Experiments on bar No. **220** *B. Manufactured by the Salisbury Iron Company, at Salisbury, Connecticut. The ore obtained from "Ore Hill," in that town. The kind of pig employed, the* mottled. *Refined after the English method. A loup made and a gun bar formed*

Marks.	Breadth before trial.	Thickness before trial.	Area before trial.	Marks.	Breadth after trial.	Thickness after trial.	Area after trial.	No. of the exp't.	DATE.	Area of section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.
0	.752	.220	.165440	Measures taken after the first experiment.									
1	.756	.218	.164808						1833.				
2	.756	.218	.164808	No. 5	.697	.199	.138703	1	Feb. 21,	.163296	63.5°	336	10080
3	.756	.216	.163296	Smallest at beginning.									
4	.756	.217	.164052	8	.719	.208	.148833						
5	.752	.217	.163184	Largest at beginning.									
6	.754	.217	.163618	24½	.697	.197	.137309						
7	.757	.219	.165783	Smallest section at present.				2	"	.164797	575	336	10080
8	.754	.218	.164372										
9	.756	.218	.164808										
10	.756	.218	.164808					3	"	.164749	62.5	336	10080
11	.753	.218	.164154										
12	.754	.218	.164372					4	"	.164749	576	347	10410
13	.754	.218	.164372										
14	.754	.218	.164372					5	"	.164907	572	361	10830
15	.756	.218	.164808					6	"	.164749	580	371	11130
16	.754	.218	.164372					7	"	.164530	67	389	11670
17	.754	.218	.164372					8	Feb. 23,	.164907	67	394	11820
18	.754	.219	.165126					9	"	.165126	67	397	11910
19	.754	.218	.164372	Measurements taken after the sixth experiment.				10	"	.164797	67	397	11910
20	.754	.219	.165126										
21	.754	.218	.164372	21	.682	.196	.133672	11	"	.165124	71.5	360	10800
22	.754	.219	.165126	22	.697	.200	.139400						
23	.754	.219	.165126	23	.677	.196	.132692	12	"	.163329	550	364	10920
24	.752	.219	.164688	24	.665	.191	.127015						
25	.753	.219	.164907	25	.668	.193	.128924	13	"	.164808	590	364	10920
26	.753	.219	.164907	26	.707	.200	.141400						
27	.753	.218	.164154	27	.717	.206	.147702	14	"	.165077	61	388	11640
28	.753	.219	.164907	28	.713	.203	.144739	15	"	.164481	61	371	11130
29	.753	.219	.164907					16	"	.164590	61.25	385	11550
30	.752	.219	.164688					17	"	.164372	61.25	385	11550
								18	"	.164372	61.25	393	11790
Mean of 27			.164587										
Maximum			.165783						Mn. of 18	.164598			
Minimum			.173184										
Mn. of the 2			.164483										
Diff. of the 2			.002599										

TABLE LXXXIV.

expressly for these experiments. Drawn under the hammer, reduced by filing to a nearly uniform size and gauged at every inch, from 0 to 30. Specific gravity, 7.7855.

Friction.	Effective strain.	Strength in pounds per square inch.	Weights producing elongation.	Elongation after being relieved from each wht.	Point of fracture.	REMARKS.
504	9576	58642	180 184 284 289 336	1st perman. 1-40 in. in 24 24 bec. 26.5 " 27. Broke.	No. 3	The bar was too long to be all put in. Took the part between 0 and 24 being the most uniform—the first elongation was under 23-48 of the breaking weight.—Very uniformly stretched.
504	9576	58108			29½	In tin from 23½ to 26½. Broke near the wedges,—this part not strained in the first experiment. Broke very soon under the same weight as before—probably would have broken with a little less.
504	9576	58125			17½	Weight put on gradually.
520	9890	60030			19½	From 22½ to 25½ now in tin. Broke near the wedges.
541	10289	62393			28½	Fracture at the opposite end to the preceding.
556	10574	64182			20½	Broke at the opposite end to the preceding.
583	11087	67386			27½	Opposite end to the preceding.
591	11229	68093			26	Same end as the preceding.
595	11315	68523			22	Opposite end to the preceding.
595	11315	68660			24½	This exper. finished this part of the bar.
540	10260	62135			0½	Not heated. Piece broken off in first exp.
546	10374	63516			5½	Piece now in from 3 to 17½. Part in tin from 8 to 12. Greatest wt previously borne 336.
546	10374	62946			10	Broke in tin with the same weight as it bore in the preceding experiment.
582	11058	66987			7½	Part now tried from 5½ to 10.
556	10574	64287			10½	Part now in from 10 to 17½.
577	10973	66669			15½	Do. from 10½ to 17½.
577	10973	66757			13½	Do. " 10½ to 15½.
589	11201	68144			12½	Do. " 10½ to 13½.

The mean area of 18 sections of fracture is .000011 square inch *greater* than that of the 31 measured sections.

TABLE LXXXV.

Experiments on bar No. 221 A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "Ore Hill," in that town. The kind of metal *employed,* "white pig," *—refined after the English method. A loup made and a gun bar*

Marks.	Breadth.	Thickness.	Areas before trial.	No. of the exp't.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.	Effective strain.
1	.757	.224	.169568		1833.						
2	.752	.227	.170704	1	Mar. 14,	.170931	570.°	318	9540	477	9063
3	.754	.229	.172666								
4	.752	.228	.171456	2	"	.172060	580.	381	11430	571	10859
5	.753	.229	.172437								
6	.753	.229	.172437								
7	.753	.228	.171684	3	"	.171031	520.	362	10860	543	10317
8	.753	.228	.171684								
9	.753	.229	.172437								
10	.753	.229	.172437	4	"	.172186	73.	404	12120	606	11514
11	.753	.227	.170931								
12	.753	227	.170931	5	"	.172437	72.	404	12120	606	11514
13	.752	.227	.170704								
14	.753	.228	.171684	6	"	.169947	71.5	404	12120	606	11514
15	.753	.228	.171684								
16	.753	.227	.170931	7	"	.171456	71.	404	12120	606	11514
17	.751	.228	.171228								
18	.752	.227	.170704	8	Mar. 16,	.170931	78.5	377	11310	565	10745
19	.753	.227	.170931								
20	.753	.227	.170931	9	"	.171684	78.	380	11400	570	10830
21	.753	.227	.170931								
22	.753	.227	.170931								
23	.753	.227	.170931	10	"	.170931	78.	380	11400	570	10830
24	.753	.228	.171684								
25	.753	.228	.171684								
$25\frac{6}{10}$	.752	.228	.171456	11	"	.170931	77.	392	11760	588	11172
Mean of 26 =			.171376	12	"	.170817	77.	392	11760	588	11172
Maximum =			.172666	13	"	.170966	76.5	401	12030	601	11429
Minimum =			.169568								
				14	"	.179931	76.	394	11820	591	11229
Mn. of these 2			.171117								
					Mean of 14	.171231					
Diff. of the 2			.003098								

TABLE LXXXV.

formed expressly for these experiments. Drawn under the hammer, subsequently reduced by filing to a nearly uniform size, and gauged at every inch. Specific gravity, 7.8018.

Strength in lbs. per square inch.	Point of fracture.	REMARKS.
53021	No. 21	Part in tin from 2½ to 5½.
63111	" 8½	Same part in tin. Temperature had been as high as 600° for a short time.
60322	" 13⅓	Broke in tin. Part immersed from 13 to 16, inclusive. Had been strained in experiment No. 1. Temperature had been 550°.
66869	" 6½	Part left in experiment 2d., which did not break *in tin.* Now broke at a part which was of a straw colour.
66772	" 5	Broke with the same weight as above.
67750	" 1½	Bore the weight a moment, and then gave way.
67154	" 4	Bore the weight a short time.
62862	" 22¾	This part was broken off in experiment No. 1.
63081	" 25	Do.
63358	" 19	The weight is supposed to have been too great. The whole weight employed in the last experiment put on by mistake. The bar broke immediately.
65359	" 20	
65403	" 18½	Gradually gave way under the same weight as the preceding.
66849	" 17½	
65693	" 11½	A different piece from the preceding.
		The mean area of the 14 sections of fracture is .000145 square inch *less* than that of the 26 measured sections.

TABLE LXXXVI.

Experiments on bar No. 221, B. Manufactured by the Salisbury Iron Company, at Salisbury in Connecticut. The ore obtained from "Ore Hill" in that town. "White pig" *metal, refined after the English method,—a loup formed and a "gun*

Marks.	Breadth.	Thickness.	Area at the section measured before trial.		No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temp. Fah.	Breaking wht. in the scale.	Breaking weight ✕ leverage.	Friction.	Effective strain.
1	.762	.232	.176784			1833.						
2	.753	.229	.172437		1	Mar. 21,	.174347	576°	335	10050	502	9548
3	.753	.229	.172437									
4	.753	.229	.172437		2	"	.173387	574	341	10230	511	9719
5	.754	.229	.172666									
6	.753	.230	.173190		3	"	.172437	576	378	11340	567	10773
7	.754	.231	.174174									
8	.755	.231	.174405									
9	.754	.229	.172666									
10	.755	.229	.172895		4	Mar. 23,	.173420	576	392	11760	583	11172
11	.755	.230	.173650									
12	.755	.230	.173650		5	"	.173420	576	402	12060	603	11457
13	.755	.230	.173650									
14	.754	.230	.173420									
15	.754	.230	.173420		6	"	.173928	100	410	12300	615	11685
16	.755	.230	.173650									
17	.755	.230	.173650									
18	.754	.230	.173420		7	"	.174289	90	416	12480	624	11856
19	.755	.230	.173650									
20	.756	.229	.173124		8	"	.173101	68	425	12750	637	12113
21	.755	.230	.173650									
22	.755	.231	.174405		9	"	.173147	68	425	12750	637	12113
23	.755	.231	.174405	Unfiled portion near the end. Br. Th. .850 .227								
24	.754	.231	.174174		10	"	.173650	68	425	12750	637	12113
25	.752	.232	.174464									
25.6	.755	.232	.175160		11	"	.192950	68	392	11760	588	11172
Mean of 26			.173682									
					12	"	.173124	67	375	11250	562	10688
Maximum			.176784									
Minimum			.172437									
					13	"	.174812	66	375	11250	562	10688
Mean of the 2			.174610									
Diff. of the 2			.004347			Mn. of 12 —	.173589					

TABLE LXXXVI.

bar" drawn expressly for these experiments. Drawn under the hammer into two bars, each reduced by filing to a uniform size, and then marked and gauged at every inch. Specific gravity, 7.8018.

Strength in lbs. per sq. inch.	Point of fracture.	REMARKS.
54764	No. 23¼	Part in tin from 3 to 6¼.
56054	" 19½	Same part still in tin.
62475	" 4	Took hold of the bar near the pan of melted metal, leaving a part for future experiment, and compelling it to break *in* or near the hottest part. Fracture *within the tin.*
64421	" 18	Part in tin from 12 to 15½ which had been tried in the second experiment—fracture near the end.
66065	" 14¼	Broke *in the tin.*
67183	" 6¾	The part now put in is from 4 to 14, both ends of which have been broken in tin at 576°. Fracture took place outside of where it had been in the tin.
68025	" 7½	Broke at a part which had been less heated than the preceding.
69976	" 8¾	Had not been in tin.
69958	" 10⅛	Had been near the tin.
69755	" 11½	Do. This part of the specimen is now finished.
57901	" unc.	Part now in the machine is from 1 to 4. The *unfiled part* being in the wedges, the fracture took place at that part.
61736	" 20	Short piece.—Had been between two former cold fractures in experiment second.
61140	" 25⅓	This piece had likewise been tried only at a cold fracture in experiment first. No additional weight was required. The mean area of the 12 sections of fracture on the filed part was .000093 square inch *less* than that of the 26 measured sections.

TABLE LXXXVII.

Experiments on bar No. 222 A. Manufactured by the Salisbury Iron Company, at Salisbury in Connecticut. The ore obtained from "Ore Hill," in that town. The metal, a mixture of "dead gray," "lively gray," "mottled," and "white" pigs, refined after the English method, a loup formed and a gun bar drawn expressly for these experi-

Marks.	Breadth before trial.	Thickness before trial.	Areas of the measured sections before trial.	Marks.	Breadths after trial.	Thickness after trial.	Areas after trial.	No. of the experiment.	DATE.	Area of the section of fracture.	Temperature, Fahrenheit.	Breaking weight in the scale.
0	.766	.221	.169286									
1	.759	.219	.166221						1833.			
2	.759	.219	.166221					1	Mar. 21,	.168942	56°	294
3	.761	.218	.165898									
4	.762	.219	.166878									
5	.762	.220	.167640	Measures taken after the 4th experiment.								
6	.761	.221	.168181					2	"	.169286	73.5	350
7	.762	.222	.169164	1	.728	.209	.152252					
8	.764	.222	.169608	2	.735	.207	.162145					
9	.762	.222	.169164	3	.730	.209	.152570					
10	.762	.222	.169164	4	.726	.207	.150282	3	"	.169164	73.5	360
11	.759	.222	.168498	5	.694	.196	.136024					
12	.761	.222	.168942	6	.690	.200	.138000	4	"	.168942	73	367
13	.761	.222	.168942	6½	.675	.193	.130275					
14	.762	.222	.169164	7	.690	.199	.137310	5	"	.168402	572	348
15	.763	.222	.169386	8	.731	.207	.151317					
16	.762	.222	.169164	9	.725	.210	.152250	6	Mar. 23,	.169164	75	377
17	.761	.222	.168942	10	.730	.210	.153300					
18	.761	.222	.168942	11	.730	.211	.154030	7	"	.168942	75	377
19	.761	.222	.168942	12	.720	.206	.148320					
20	.762	.221	.168402					8	"	.168181	75	377
21	.761	.222	.168942					9	"	.167497	75	377
22	.762	.222	.169164					10	"	.169164	75	377
23	.762	.221	.168402					11	"	.168942	67	374
24	.762	.220	.167640					12	"	.168402	67	374
25	.761	.221	.168181					13	"	.168046	67	374
26	.762	.221	.168402									
27	.762	.221	.168402					14		.168497	67	374
27½	.762	.221	.168402									
								15		.168737	67	378
Mean of 29			.168420						Mean of 15	.168686		
Maximum			.169608									
Minimum			.165895									
Mean of the 2			.167751									
Diff. of the 2			.003713									

TABLE LXXXVII.

ments. Drawn into two bars (A & B.) Reduced to a uniform size by filing, gauged at every inch from 0 to 27½, inclusive. Specific gravity, 7.7555.

Breaking weight & leverage.	Friction.	Effective strain.	Strength in lbs. per square inch.	Point of fracture.	REMARKS.
				No.	
8820	441	8379	49597	17	Part in tin from 4 to 8. Temperature rose once above 650°.
10500	525	9975	58924	0	The temperature rose to 660° when the bar appeared to be breaking. Having allowed the temperature to abate until it descended to 575°. Added weights until it appeared to be again breaking with 335 lbs. in the scale. Some tin then escaped at the packing, and 15 lbs. more were required to break it when cold.
10800	540	10260	60651	14	
11010	550	10460	61915	12½	
10440	522	9918	58895	27½	Part in tin from 21 to 24½.
11310	565	10745	63518	7	Part under trial from 1 to 12.
11310	565	10745	63602	10½	Section of fracture gauged after this experiment, .586×.153 = .089658.
11310	565	10745	63889	6	
11310	565	10745	64150	4½	
11310	565	10745	63518	9½	
11220	561	10659	63092	18½	
11220	561	10659	63295	26	
11220	561	10659	63429	24½	
11220	561	10659	63259	22⅞	
11340	567	10773	63851	20⅝	The mean area of 15 sections of fracture, is *less* than that of 29 measured sections by .000266 square inch.

TABLE LXXXVIII.

Experiments on bar No. 222 B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "Ore Hill," in that town. The metal was a mixture of "dead gray," "lively gray," "mottled," and "white" pig. Refined after the Eng-

Marks.	Breadth.	Thickness.	Areas of measured sections before trial.	No. of the experim'ts	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.
0	.752	.217	.163184		1833.					
1	.757	.217	.164269	1	Mar. 28,	.166100	560.°	298	8940	447
2	.757	.218	.165026							
3	.757	.219	.165783	2	"	.163455	576.	308	9240	462
4	.757	.219	.165783							
5	.756	.219	.165564	3	"	.166100	566.	320	9600	480
6	.755	.217	.163835							
7	.754	.216	.162864	4	"	.166320	554.	348	10440	522
8	.754	.217	.163618							
9	.755	.219	.165345	5	Mar. 30,	.163241	566.	349	10470	523
10	.755	.219	.165345							
11	.757	.220	.166540	6	"	.163998	66.	349	10470	523
12	.757	.220	.166540							
13	.756	.221	.167076	7	"	.162864	66.	374	11220	561
14	.756	.220	.166320							
15	.756	.220	.166320	8	"	.165026	65.	374	11220	561
16	.756	.220	.166320							
17	.755	.220	.166100	9	"	.164267	65.	375	11250	562
18	.755	.220	.166100							
19	.755	.220	.166100	10	"	.165783	65.	375	11250	562
20	.755	.220	.166100							
21	.754	.218	.164372	11	"	.166100	554.	332	9960	498
22	.754	.218	.164372							
23	.754	.218	.164372	12	"	.165816	578.	345	10350	517
24	.756	.219	.165564							
25	.756	.220	.166320	13	"	.167076	64.	369	11070	553
26	.756	.220	.166320							
27	.756	.220	.166320	14	"	.166540	64.5	370	11200	555
27.7	.756	.219	.165564							
				15	April. 4,	.165753	75.5	374	11220	561
	Mean of 29		.165425							
				16	"	.165236	74.	369	11070	553
	Maximum		.167076							
	Minimum		.162864	17	"	.164372	74.	369	11070	553
Mean of these 2			.164970		Mean of 17		.165178			
Diff. of the 2			.004212							

TABLE LXXXVIII.

lish method. A loup formed and a gun bar drawn, expressly for these experiments. Reduced to a nearly uniform size by filing, and gauged at every inch from 0 *to* 27.7. *Specific gravity,* 7.7555.

Effective strain.	Strength in lbs. per square inch.	Point fractured.	REMARKS.
8493	51132	No. 18½	Part in tin from 5½ to 9.
8778	53703	" 0¼	Broke near the wedges.
9120	54907	" 17⅛	Broke near the wedges on the opposite side of tin bath.
9918	59632	" 15⅛	Do.
9947	60934	" 7½	Broke in the tin.
9947	60653	" 0¾	
10659	65447	" 7	Cut off the tinned part in the gripe of the wedges.
10659	64589	" 2	
10688	65064	" 5¾	
10688	64469	" 3⅞	
9462	56966	" 20	Part in tin from 22¼ to 25½.
9833	59301	" 24⅓	Broke *in tin.* Temp. had been as high as 614°.
10517	62947	" 13	This part not heated.
10545	63318	" 11	
10659	64389	" 24¼	Part now under trial from 20 to 24⅓.
10517	63649	" 20½	
10517	63983	" 21½	
			The mean area of the 17 sections of fracture is .000247 square inch *less* than that of the 29 measured sections.

TABLE LXXXIX.

Comparative view of the influence of high temperatures on the strength of iron, as exhibited by 73 experiments on 47 different specimens of that metal, at 46 different temperatures, from

No. of the comparison	Temperature observed at the moment of trac.	Mark of the bar on which the trial was made.	Strength at ordinary temperatures.	No. of experiments at ordinary temperatures.	Strength at the temperature observed.	No. of experiments at high temperatures.	Amount of variation from uniformity in the cold experiments.	Effects of the heat expressed in parts of the original strength.	REMARKS.
1	212°	137	56736	1	67939	1		+ .197	
2	214	133	53176	1	61161	1		+ .150	
3	394	58	68356	1	71896	1		+ .052	
4	394	148	65143	1	69752	1		+ .070	
5	394	23	62646	2	67765	1	.1041	+ .081	
6	394	125	57182	1	63322	1		+ .107	
7	394	61	55297	5	61917	1	.2026	+ .119	
8	396	75	60433	3	62415	1	.0444	+ .031	
9	440	224D.	49782	4	59085	1	.0908	+ .187	
10	520	224B.	54934	4	58451	1	.0992	+ .064	
11	550	199A.	76986	4	79846	2	.0936	+ .037	The standard for the original strength may possibly be a little too high.
12	550	221A.	60518	4	60322	1	.1680	— .004	
13	552	14	52542	1	55932	1		+ .064	
14	554	218A.	58124	4	60412	1	.0730	+ .039	
15	554	22	54372	4	61680	3	.1919	+ .134	
16	560	224E.	50528	7	58821	1	.0605	+ .158	
17	562	224C.	53385	5	59623	1	.1919	+ .104	
18	563	60	60907	4	72588	2	.0460	+ .191	
19	564	74	51030	5	58284	1	.0764	+ .142	
20	568	9	67211	2	76763	1	.0601	+ .042	Standard probably too high for the mean strength.
21	572	219B.	66724	2	66620	1	.0325	— .002	
22	572	49	59607	3	62278	1	.0878	+ .045	
23	572	222B.	56165	4	60117	2	.1550	+ .070	
24	573	10	64511	1	67503	3		+ .046	
25	574	231	76071	5	65387	1	.1373	+ .014	
26	575	220A.	54263	4	60988	1	.0280	+ .124	
27	575	62	58376	3	70081	3	.0262	+ .200	
28	575	207	51924	5	63825	3	.1225	+ .229	
29	576	221B.	59234	5	66065	1	.1190	+ .115	
30	576	223B.	43386	6	50068	1	.0760	+ .154	

TABLE LXXXIX.

212° to 1317° Fah., compared with the strength of each bar when tried at ordinary temperatures, the whole number of experiments at the latter being 163.

No. of the comparison.	Temperature observed at the moment of frac.	Mark of the bar on which the trial was made.	Strength at ordinary temperature.	No. of exp. at ord. temp.	Strength at the temperature observed.	No. of exp. at high temp.	Amount of variation from uniformity in the cold experiments.	Effects of the heat in parts of the original strength.	REMARKS.
31	°577	164	58769	5	66929	2	.1214	+.139	
32	578	224A.	52406	5	59197	1	.0565	+.129	
33	578	223A.	45757	5	53465	1	.0896	+.168	
34	580	86	62156	3	77163	2	.0986	+.052	
35	590	220B.	59459	5	62966	1	.0680	+.058	
36	598	90	50316	5	57310	2	.2401	+.138	
37	630	219A	59530	4	60010	1	.1440	+.008	This experiment was on a part probably defective.
38	636	16	53543	1	50039	1	.1563	—.067	
39	662	150	59307	5	58181	1	.0644	—.019	
40	722	152	57133	3	54441	1	.0507	—.047	
41	732	14	52542	1	53378	1	.1310	+.016	
22	734	150	59397	1	57903	1	.0644	—.026	
43	766	16	56891	1	54819	1	.1563	—.037	
44	770	149	56825	2	54781	1	.0234	—.036	
45	824 and 814	214	59219	1	55892	1	.0413	—.073	
46	825	149	56825	2	56644	1	.0234	—.029	
47	932	214	59219	1	45531	1	.0413	—.240	
78	947	232	58341	2	42401	1	.0446	—.273	
49	1022	214	59219	2	37410	1	.0413	—.369	
50	1037	152	58992	1	37764	1	.0507	—.360	
51	1097	227	53426	6	27604	1	.0330	—.483	
52	1111	227	53426	6	27602	1	.0330	—.483	
53	1142	226	54758	2	18672	1	.1147	—.659	The metal was decidedly defective at the point where this fracture was made —flaws visible.
54	1155	227	53426	6	21967	1	.0330	—.589	
55	1159	229	55774	3	25620	1	.1102	—.538	The 8th experiment on this bar being taken as the standard would exhibit the effect—.550.
56	1187	227	53426	6	21910	1	.0330	—.589	
57	1235	226	54758	2	21298	1	.1147	—.611	
58	1245	226	54758	2	20703	1	.1147	—.622	
59	1317	226	54758	2	18913	1	.1147	—.654	
		Mean	57525						

Effect of high temperature on iron.

The experiments on bars of iron at high temperatures, were made either on sections deeply filed, or on those specimens which had been reduced by filing to a uniform size.

The trials below 600° were chiefly conducted in a bath of oil, arranged round the bar as already represented in Plates III. and IV., and the temperatures marked by the mercurial thermometer. For temperatures above that point the bath of tin and lead was substituted, and, when necessary, the steam pyrometer took the place of the common thermometer.

The view already presented of the influence of heat on copper, indicated partly by each of these two instruments, has enabled us to observe that they connect themselves in their indications in a manner to prove that no serious errors can be anticipated in the temperatures assigned in the higher parts of the scale when operating on iron.

If, however, in examining the effect of temperature on copper we meet with some difficulties in consequence of the irregularities of structure in the material, of want of conformity in different bars, and of the occasional weakening effects of alloying, on the total tenacity as we approach a red heat, the obstacles there encountered are comparatively trifling, when contrasted with those which are to be surmounted in the investigation of the effects of heat upon the tenacity of iron. Here we have, not only the variations due to the original composition of the metal; the differences resulting from the variety of pig-metal used in its manufacture, and the defects of the mechanical structure, owing to the want of uniformity in welding, or of regularity in the temperature of working the bars; but we have superadded to all these, a singular anomaly in the effect of heat itself on the tenacity of this material, which is believed never to have been before made the object of special inquiry.

Notwithstanding these impediments, the committee have not felt authorized to leave so important a point of inquiry, without a faithful attempt to unravel its intricacies. It would have been easy to devise a set of experiments, which, for a theoretical purpose, might have afforded to the analyst some interesting problems, and probably served to clear the subject of heat from certain difficulties with which its investigation is encumbered. Such, however, was not the purpose in view of the committee.

When we attempt to form a scale of the weakening effects of elevated temperatures, founded, as in the case of copper, on trials at ordinary temperatures, or even at the freezing point, we shall find that many of the first numbers in the scale will be negative, instead of positive, and this will continue to different points of temperature, according to the nature or condition of the iron on which the experiments are made. In fact, some of the very first experiments at high temperatures rendered this manifest, by showing that on a bar of uniform size, the fracture would not take place within the heating bath; and even that *much* filing of the part in the oil or melted metal, was necessary in order to prevent the fracture from taking place at unfiled sections *out of the hot bath* rather than at the filed one in it. This circumstance was noted at 212°, 392°, and 572°, rising by steps of 180° each from 32°, at which last point some trials had been made in melting ice. At the highest of these points, however, it was perceived that some specimens of the metal exhibited but little, if any, superiority of strength over that which they had possessed when cold, while others allowed of being heated

nearly to the boiling point of mercury before they manifested any decided indication of a weakening effect from increase of temperature.

It hence became apparent that any law, taking for a basis the strength of iron in its ordinary condition, and at common temperatures, must be liable to great uncertainty, in regard to its application to different specimens of the metal. It was evident that the anomaly above referred to, must be only apparent, and that the tenacity actually exhibited at 572°, as well as that which prevails while the iron is in the state in which it was left by forging, or rolling, must be below its maximum tenacity. To determine what ratio exists between the ordinary strength of a bar and its maximum strength when in the most favourable condition for resisting a longitudinal strain, experiments were made on several bars by heating them to 572°, and then applying weight enough to cause a fracture, either within or without the heated part. The bar was then taken out and allowed to cool, when the strength which was obtained on parts influenced by the heat became a standard of comparison for experiments at more elevated temperatures. A mean of thirty-five comparisons, conducted in the manner just described, afforded a standard 16.2 per cent. greater than the ordinary strength of the metal; but the standard most relied on for furnishing the basis of calculations, and for determining a law of diminution of tenacity, was derived from the five varieties of iron, manufactured by the Salisbury Iron Company, which, being of a tolerably uniform texture, were considered rather more suitable than others for supplying the ground work of a law for calculating the effect of temperature on this metal generally. An examination of the trials on those bars will be found to furnish a standard of maximum tenacity 15.17 per cent. greater than their mean strength when tried cold. When, however, an unexceptionable standard was given by any bar after trial at 572° and subsequent cooling off, its own standard for increased strength was used in computing the true effect of heat at other high temperatures.

Thus, at a temperature of 1317°, the bar No. 226, which had possessed, when cold, a strength of 54758 lbs., gave a remaining strength of only 18913 lbs. Now, 54758 lbs. increased 15.17 of itself, gives 63065, and from this deducting 18913 we have 44152 lbs. for the *diminution* of its absolute tenacity by the temperature just mentioned, or .7001 of the maximum strength.

On the same bar, (No. 226,) were made at different points, two other experiments with the same weight each time in the scale.

The first of these sections gave way when the temperature had reached 1237°. The strength per square inch given in this case was 21298, and comparing this with the maximum strength, 63065, we obtain 41767 as the diminution, equal to .6622 of that maximum.

The second trial on a larger area of section required a higher temperature to cause the fracture to take place under the given weight, viz: 1245°, giving at this temperature a tenacity of 20703 lbs., and by the same computation showing a diminution from the maximum 63065 of .6715. Both of these trials having been made with the precaution of raising and lowering the suspended furnace, to regulate the heat, it is believed that no essential error in regard to temperature can have existed. The first was conjectured to be, if anything, a trifle in excess.

If we take the mean of these two results, viz. .6668 for the diminution of tenacity at 1241° the mean temperature, it cannot vary far from the true

effect. On bar 227 an experiment was made at 1187°, giving a tenacity of 21913 lbs. per square inch. Within two and a half inches of the same point a cold fracture gave a strength of 52186 lbs., from which the calculated maximum is 60102, and the diminution is 60102—21913, or 38189; which is .6352 of the same maximum tenacity.

On No. 229 was made an experiment at 1159°, which exhibited a tenacity of 25620 lbs. Three experiments on the same bar when cold, gave a mean strength of 55774 lbs. Hence 55774×.1517=8460; and (55774 +8460)—25620=38614, which is .6011 of the maximum tenacity.

On No. 227 we have an experiment at 1155°, giving a tenacity of 21967, and the four cold experiments nearest to the same point give a mean of 47749, from which we obtain the maximum 54992, and the diminution = .6000.

On No. 226 was made an experiment at 1142°, but as the iron at the part in which the fracture took place was defective from flaws, and had probably been impaired by the previous straining of the bar, it was not considered necessary to attempt to reduce its apparent tenacity to the standard, being entirely anomalous.

At 1111° the bar No. 227 had a strength of 27602, and another trial on the same at 1097°, 27602.

The weight in the scale was the same in both cases, and the temperatures would probably have been the same, had not the standard piece in the latter case accidentally risen above the melted lead a short distance just before the fracture. Taking the mean of these two results 27603, for the strength at 1111°, and the mean of six trials on this bar near the two points where these fractures occurred, viz: 53426, we obtain the maximum tenacity at those points 61531, and the diminution by heat .5614.

On bar 152 an experiment at 1037° gave a tenacity of 37764, and on No. 214 an experiment at 1022° gave 37410. The mean cold strength of these two bars was 59105, from which we deduce the maximum 68071; and the diminution for the mean temperature 1030°, equal to .4478 of the maximum.

At 947° an experiment on bar No. 232 gave a strength of 42401, the mean of the two experiments subsequently made nearest to this point gives the experimental maximum strength 66193 from which the diminution is .3593.

At 932° bar No. 214 had a tenacity of 45531, while its cold strength was 59319, and its maximum 68202, hence the diminution is .3324.

An experiment was made on bar 149 at a temperature marked 825°, but as the furnace was not lowered during the performance of it, and as the time during which the bar continued to stretch after the strength had been fairly overcome, was considerable, the temperature is in all probability too high; and the experiment is not considered comparable with the rest of the series.

In bar No. 214, at the temperature of 824°, the remaining strength was 55892, the original strength, 60850, and the maximum by calculation, 70080, whence the diminution is .2010.

On bar 149 was an experiment at 770°, giving a tenacity of 54781; while the original strength was 56825, and the diminution from the calculated maximum .1627.

On No. 16 we find an experiment at 766°, giving 54819. Two subsequent experiments yielded maxima, the mean of which is 65176, whence the diminution is .1586.

In bar No. 150, a temperature of 734°, left a strength of 57903. The first experiment on the bar afforded 59397, from which we calculate the maximum 68407, which proves the diminution at this temperature to be .1535.

On No. 14 we obtained a strength of 53378, at 732°, and the mean of three experimental maxima, is 62736, hence the diminution by heat is .1491.

On No. 152 we had at 722° a tenacity of 54442, and three experiments gave a *cold strength* of 55990, from which a calculated maximum of 64483 is obtained, and consequently a diminution of .1557. But an experimental maximum of 62709 was obtained on this bar, which on account of the remoteness of the point where it occurred, from the point on which the hot fracture was made, is believed to be rather too low. Calculating, however, from this maximum, we find the diminution .1316.

If we take the mean of the two results, .1557 and .1316, we have the probable diminution from the true maximum, .1436.

On No. 150 we find an experiment at 662°, giving a tenacity of 58182. On the same bar an experimental maximum was found of 65785, from which we get the diminution equal to .1155.

On No. 16 was made a trial at 636°, yielding a result of 50039, a result far lower than that given afterwards on the same bar at 766°; we are therefore compelled to believe that this experiment was made on a defective part of the bar.

On No. 219 A, was a trial at 630°, which exhibited a tenacity of 60010. An experiment subsequently made within 1¾ inches of the same point, gave a tenacity of 67033, and consequently the diminution is .1047.

On No. 90 an experiment at 600° gave a tenacity of 56938, and three experiments on the same bar, when cold, gave a mean of 54715, from which the calculated maximum is 63015, and the diminution .0964.

On the same bar (No. 90,) another trial took place at 596°, giving a strength of 57682 lbs., and if we assume the original strength of this section equal to that given by the third experiment on the same bar, 55037, we shall have the maximum by calculation 63386, and the diminution .0899.

By a mean of 5 sets of experimental maxima derived from 65 trials on the 5 varieties of Salisbury iron we have a standard of 66146. The six trials at the mean temperature of 570° referred to in our remarks in Table LXXIX. of the effect of employing different kinds of pig-metal, show that at a mean temperature of 570° those trials gave a strength of 60398 lbs., whence the diminution is .0869.

Of 224 B, at 520° the tenacity was 58451. On the same bar, four cold experiments gave a mean strength of 54934, which by calculation gives a maximum of 63267 and a consequent diminution of .0761.

On a survey of the preceding discussions it will be seen that in determining the maximum belonging to each point of fracture, it has been necessary to resort sometimes to experimental, and sometimes to calculated results, but that in several cases the two operate as checks upon each other.

On attempting to extend the principle to trials made below the temperatures already cited, we are liable to encounter an ambiguity in the results, owing to the fact that the maximum tenacity is not generally to be obtained without having carried the previous temperatures to about 550° or 600°, and the tension to nearly or quite that of the original strength of the metal when cold.

In projecting into a curve as in Plate X. the data furnished by the experiments above described, and of which a synopsis is given in the following table, it becomes at once apparent that what was *conjectured* with respect to copper, in regard to a point of inflection, is here presented in a manner to admit of no uncertainty. Indeed it could hardly be otherwise, when we consider that the melting point of wrought iron, at which all tenacity must be overcome, is doubtless situated above 3000° ; and by the experiments of Clement and Desormes, is as high as 3945°. Now it appears that at a temperature no higher than about 1050° one-half of the strength is destroyed ; at 1240°, two-thirds ; and at 1317°, seven-tenths of the maximum tenacity is overcome.

The following table exhibits the observed temperatures, and corresponding tenacity of the metal with the calculated, or experimental maximum of strength,—the ratio of the observed diminution to the maximum tenacity, and the irregularity of the metal in parts of the original strength at ordinary temperatures.

TABLE XC.

No. of the comparison.	Marks of the bar.	Temperature observed.	Tenacity observed.	Maximum tenacity at the point of fracture.	Manner in which the maximum was obtained.	Diminution by heat in parts of the maximum tenacity.	Irregularity of the metal in parts of the original strength.
1	224 B.	520°	58451	63275	Experiment.	.0738	.0992
2	Salisb. iron.	570	60398	60398	do.	.0869	.1125
3	90	596	57682	57682	Calculation.	.0899	.2401
4	90	600	56938	63086	do.	.0964	.2401
5	219 A.	630	60010	67033	Experiment.	.1047	.1440
6	150	662	58182	65785	do.	.1155	.0644
7	152	722	54442	64483	Calculation.	.1436	.0507
8	14	732	53378	62736	Experiment.	.1491	.1310
9	150	734	57903	68407	Calculation.	.1535	.0644
10	16	766	54819	65176	Experiment.	.1589	.1563
11	149	770	54781	65445	Calculation.	.1627	.0234
12	214	824	55892	70080	do.	.2010	.0413
12	214	932	45531	68202	do.	.3324	.0413
14	232	947	42401	66193	Experiment.	.3593	.0446
15	{214 / 152}	1030	37587	68071	Calculation.	.4478	.0460
16	227	1111	27603	61531	do.	.5514	.0330
17	227	1155	21967	54992	do.	.6000	.0330
18	229	1159	25620	64234	do.	.6011	.1102
19	227	1187	21913	60102	do.	.6352	.0330
20	226	1237	21298	63065	do.	.6622	.1147
21	226	1245	20703	63065	do.	.6715	.1147
22	226	1317	18913	63065	do.	.7001	.1147

From the eighth column of the preceding table it appears that of these 15 different specimens of iron, the mean irregularity of structure is 10 per cent. of the mean strength when tried cold.

For the purpose of ascertaining, approximately, the law of decrease in strength by temperature, an investigation was made similar to that adopted for copper, embracing, however, only 12 of the points contained in the preceding table.

As some of the experiments which furnished the standards of comparison for strength at ordinary temperatures, were made at 80°, and as at that point small variations in respect to heat appear to affect but very slightly the tenacity of iron, it was conceived that for practical purposes at least, the calculations might be commenced from that point.

Eighty degrees are therefore deducted from each temperature in the following table, and the remainders used, instead of the numbers commencing from the 0 of our scale. It will be found that with the exception of a slight anomaly between 520° and 570°, amounting to —.08, the numbers expressing the ratio between the elevations of temperature, and the diminutions of tenacity, constantly increase until we reach 932°, at which it is 2.97, and that from this point the ratio of diminution decreases to the limits of our range of trials, 1317°, where it is 2.14. It will also be observed, that the diminution of tenacity at 932°, where the law changes from an increasing to a decreasing rate of diminution, is almost precisely one-third of the total, or *maximum* strength, of the iron at ordinary temperatures.

At this point it will be seen, the curve traced in the figure, Plate X., undergoes an inflection, and in all probability continues in the same general direction to the fusing point.

TABLE XCI.

No. of the comparison.	Observed temperatures.	Observed temperatures — 80°.	Observed diminution of tenacity.	Power of the temperature which represents the diminution of tenacity at each point.	REMARKS.
1	520°	440°	.0738	2.25	
2	570	490	.0869	2.17	
3	596	516	.0899	2.38	
4	662	582	.1155	2.67	
5	770	690	.1627	2.85	
6	824	744	.2010	2.94	
7	932	852	.3324	2.97	Point of inflection near this temperature.
8	1030	950	.4478	2.92	
9	1111	1031	.5514	2.63	
10	1155	1075	.6000	2.60	
11	1237	1157	.6622	2.41	
12	1317	1237	.7001	2.14	
			Mean	2.58	

From the above table it appears that the ratio of diminution furnished by a comparison of some of the lower temperatures with all those above them is higher than the duplicate. The same inference is derived from a comparison of the higher members of the series with all those below them.

At 932° it will be seen that a comparison with all those both above and below that temperature, gives a rate very nearly approaching to the cube. The particular comparison between 824° and 932° gives a rate higher than the 4th power, viz. 4.08.

Hence though the diversity of the metals operated on, is such as not readily to furnish the precise mathematical law, it is still abundantly apparent that this law must be different from that which is indicated by any one of the family of parabolas.*

But for practical purposes the table indicates, by the *mean* of all the rates, that a rule may be followed, not widely different from what is represented by saying that the *thirteenth power of the temperature above* 80° *is proportionate to the* 5*th power of the diminution from the maximum tenacity.*†

Plate X. exhibits at .1517, in the line of observed diminution, the commencement of a branch of the curve descending to the right, which indicates the progressive effects of temperature, increasing, as it rises, the tenacity of iron until a certain point is reached, when the weakening influence begins to be felt. The other branch of the curve, or that which takes its rise from the origin of the abscissas, forms, with the first, a cusp of peculiar character at a point *c*, which, however, the experiments are not sufficiently numerous in this part of the scale to determine exactly in respect to *position*.

From the preceding discussion Table LXXXIX. will be sufficiently intelligible without further comment.

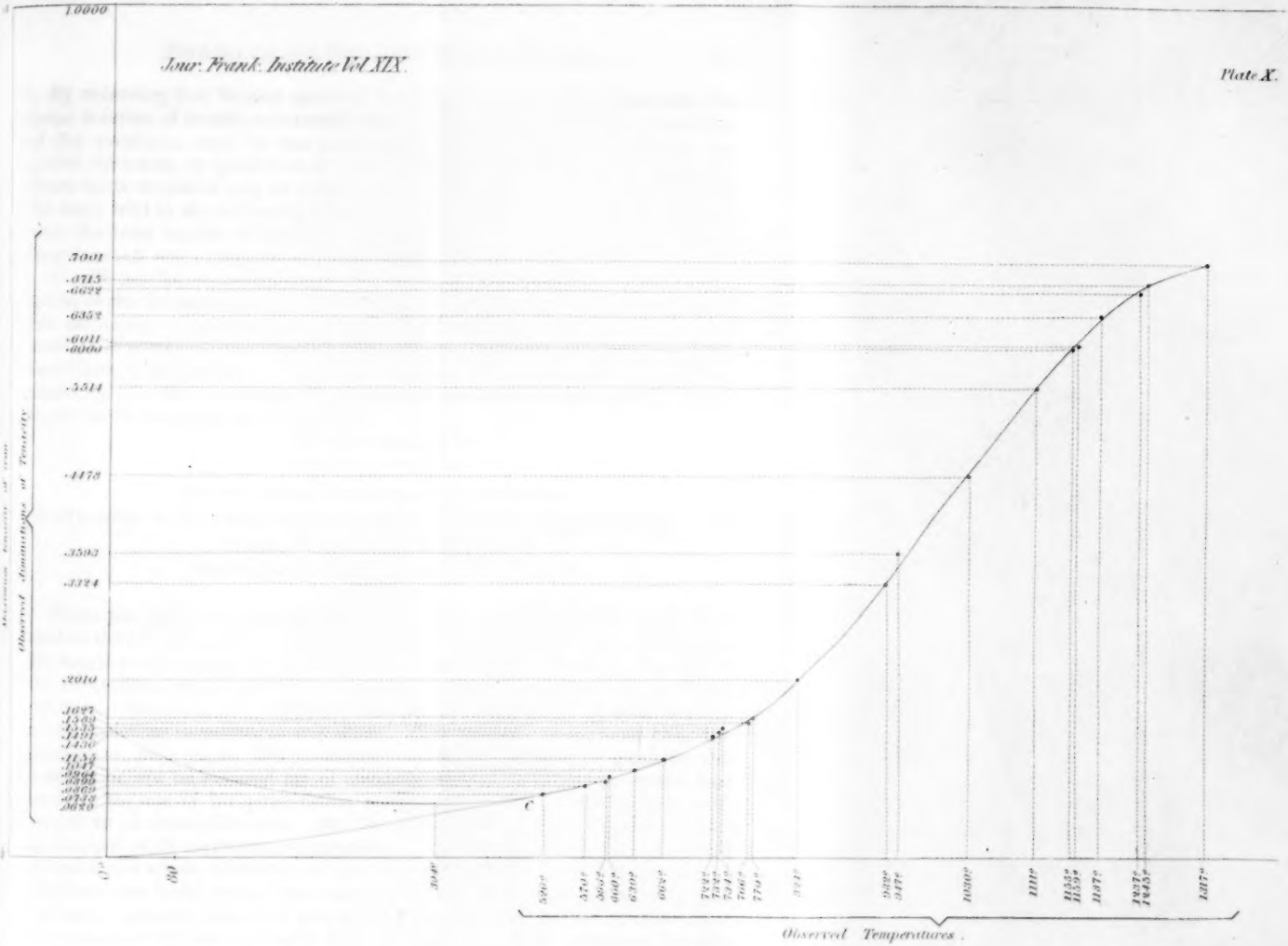
Elasticity of Iron.

In describing the method of determining the elasticity of the *machine* for tenacities, we have given in effect a detail of the processes also pursued with the bars of iron generally.

The strong bar then interposed between the heads, *b'*, *b''*, (Plate I.) was, however, now replaced by the specimen under trial, and as the machine was capable of overcoming the total strength of each specimen, the temporary elongations, as well before as after the bar had begun to be permanently extended, were easily deducible from the observed elasticity of the *bar and of the machine together*, corrected by deducting the already ascertained elasticity of the machine alone.

* Represented by the well known general formula, $y^q = mx^p$.

† It is evident from what has already been said, that if we would *calculate* the reduction from the ordinary strength of iron as it comes from the hammer or the rolls, we must first reduce it to the maximum, by adding to its observed strength at a known low temperature, 15.17 per cent. of itself. It is, moreover, apparent from the curve, as well as from the table, that if we conceive the ordinates (y) or increments of temperature, and the abscissas (x) or decrements of tenacity to be drawn from any two points *below* 932°, the increase (dx) of any abscissa, for the contemporaneous increase (dy) of the corresponding ordinate, will observe an *increasing rate;* but *above* that point where also the *differential co-efficient* $\frac{dy}{dx}$ is a maximum, and where the *differential co-efficient of the second order* $\frac{d^2y}{dx^2}=0$, they observe a decreasing rate and the concavity of the curve is accordingly directed towards the axis of the abscissas, in place of the convexity which had hitherto inclined in that direction.



that in his engine the steam is always pressing on one side or the other of the large piston, and always pressing on *both* sides of the small one, thereby creating great reaction on the small piston. Each end of his large cylinder is alternately open to the condenser, the small one never.

and where the *differential co-efficient of the second order* $\frac{d^2y}{dx^2}=O$, they observe a decreasing rate and the concavity of the curve is accordingly directed towards the axis of the abscissas, in place of the convexity which had hitherto inclined in that direction.

By referring to a former part of this report (Table III.) there will be found a series of numbers corresponding to the several observed elasticities of the machine, used by the committee, and representing in inches the actual distances, or quantities of recoil after the strain had been removed. From those numbers may be taken out *by inspection*, the quantity of recoil for each trial in the following table, and comparing the results thus obtained with the total lengths of the bars, it will be seen by what part of its whole length, each was elongated and contracted at every trial.

It will not fail to be remarked that in the more extended series of the table, those for example, in which 7 or 8 trials were made on the same bar, the maximum of elasticity was often found within a comparatively small number of pounds of the *breaking weight*, and that it was seldom so low as two-thirds of that weight. This is at variance with the supposition that the elasticity of a bar is destroyed or much diminished at the moment it has begun to be permanently elongated.

(TO BE CONTINUED.)

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Observations on the duty performed by the Cornwall Steam Engines. By JACOB PERKINS, Civil Engineer.

Read before the Institution of Civil Engineers.

No. III.

When the paper on the expansive property of highly elastic steam, was read on the 8th inst., and accompanied by a sectional drawing of a double cylinder single-stroke engine of my invention, it was said by one of the members of the Institution, that it was Woolf's engine which I had produced as being my own. Now, sir, it so happens that the two engines in question are entirely dissimilar in principle and effect. It is difficult to believe that the gentleman who made this declaration took the trouble to examine the drawing before he accused me of claiming another person's invention; had he done so, one of his penetration would, at a glance certainly, have perceived an obvious difference. Mr. Woolf's engine was one of great promise on account of its ingenious arrangement. It was supposed that it would embrace the whole advantage of the expansive property of high steam. It however was found that if the same steam had been admitted directly into the large cylinder instead of first passing through the smaller cylinder, that more duty would have actually been performed. This unlooked for fact confounded the supporters of this engine and puzzled every one. To trace out the cause of this unexpected result we have only to imagine the small cylinder to be filled with steam of, say 40 lb. per inch, the steam also to be let into the large cylinder at the same pressure, then the small piston would be neutralized and would have no tendency to move in any direction, but since the steam could find its way between the large piston and the head of the large cylinder, both pistons will begin to move in the same direction. It will be seen that the steam which is acting on the large piston is reacting against the small one, and were both the pistons of a size no movement would take place.

The most important distinction between Mr. Woolf's engine and mine is that in his engine the steam is always pressing on one side or the other of the large piston, and always pressing on *both* sides of the small one, thereby creating great reaction on the small piston. Each end of his large cylinder is alternately open to the condenser, the small one never.

In my arrangement it will be seen that the steam is never on both pistons at the *same time*, for one piston or the other is always working *without* steam pressing on it. Another trait which belongs to no other engine within my knowledge is, that of the large cylinder being *always* open at one end to the condenser, and the small one half the time to it, producing the important result of removing *all* cause of reaction. I think it cannot but be acknowledged that there is difference enough in action and effect to constitute mine an entirely different engine.

Mr. J. Hornblower was undoubtedly the first who used the double cylinder expansive engine. This engine failed, however, and Mr. Woolf comes next with his double cylinder expansive engine. Mr. W.'s engine although upon the same general principle as Mr. H.'s, possessed much more power, and was of course much improved. This was unquestionably owing to Mr. Woolf's using much higher steam, and his having constructed a very ingenious boiler, which generated much more steam (if attended by a skilful stoker,) than any other kind of boiler then extant. It was found, however, that in practice, too much attention and skill was necessary to make it profitable, and it was given up. A few days' work was enough to destroy a new boiler if not properly attended to; the repellant property of heat was more perceptible in this boiler than in any other. Mr. Woolf, in answer to some questions put to him, admitted that, at times, the water entirely left the small tubes and rested in the steam chamber, making at the same time very little steam, and his small tubes would then either crack or melt. This, he observed, was fatal to his experiment, which at first promised so much.

According to the Cornwall reports, Mr. Woolf's engine, at one time, raised 57,000,000 lbs. one foot high with a bushel of coals, while at the same time the utmost duty done by Mr. Watt's single-stroke, balance-bob, expansive engine was 33,000,000 lbs.

If Mr. Woolf had dispensed with the little intermediate cylinder altogether, his engine would have done much more duty; for his large cylinder worked with 40 lbs. steam expansively, and his boiler generated much more steam at times than the present Cornish boiler. Mr. Woolf soon abandoned his smaller cylinder, which showed that the practical trial had taught him its inutility.

There is no gain that can mathematically be estimated in the use of two cylinders, and as long as the pressure of the steam falls short of about 200 lbs. per inch, a single cylinder will operate much better, and will produce all the advantages to be obtained by the expansive property of high steam. The only use of the small cylinder is that of taking off the strain upon the engine when the pressure exceeds a certain point. It will be seen that if the steam had been let on to the large piston at 600 lbs. the engine must have been inconveniently strong to have borne the strain. It would have been impracticable in such a cylinder, to have cut off the steam at $\frac{1}{3\frac{1}{5}}$ part of the stroke, but by the use of the small cylinder, I was enabled to allow the engine to make one-fourth of the stroke before the steam was stopped off, giving time enough to remove all practical difficulties.

You say, observed one engineer, that Woolf lost much power by using two cylinders, and yet you try to prove that with a little alteration, or modification, there is a great advantage in using two; I should like to know how this is? I still maintain that one cylinder is far better than two where steam as low as 40 lbs. per inch is used. I by no means would recommend two cylinders, unless the steam to be used is considerably above 200 lbs.

It will be seen that Woolf did not use his intermediate cylinder to soften down the stroke, for the steam was admitted on to the large piston at the same pressure that it entered the small one.

I have thus endeavoured to point out the causes of the failure of these two very ingenious engineers in the novel attempt of applying the valuable power arising from the expansive power of steam through the agency of two cylinders, but I hope that it will not be believed that I have done so from any unworthy motive. Greater credit is due to the originator of a new principle, although he fail in realizing his expectations, than to the more fortunate individual who through his errors sees the cause of his failure, and benefits by his want of success. It is by observing our own errors, and those of others, that we are enabled to arrive at truth.

JACOB PERKINS.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Observations on the circumstances attending the Explosion, and Bursting of Steam Boilers; with animadversions on the Experiments of the Franklin Institute, on that subject. By JACOB PERKINS, Civil Engineer.

A series of experiments to discover the cause of the explosions of steam boilers, was ordered to be made, by the Secretary of the Treasury, of the United States of America, and the Franklin Institute was selected to make the experiments; and certainly no pains have been spared by that Institution in its researches; but from the want of some practical facts which have transpired in the course of the experiments which I have made within the last twelve years, they have, as I believe, arrived at some conclusions entirely fallacious, and which will, if not controverted, tend to injurious consequences. The most dangerous of these conclusions is, that all the destructive explosions of steam boilers, have been caused by the direct pressure of the steam, and that there is no difference between the explosion, and the bursting, of a steam boiler. If the late Oliver Evans, (who in his own department was one of the greatest men America has ever produced) had been still living he would have given a very different opinion. Mr. Evans had a record of more than 600 burstings, before he had one explosion, although the pressure was greater when the bursting took place, than when the explosion happened. I have myself witnessed enough to be perfectly satisfied that there is as much difference between a bursting and an explosion of a steam boiler, as there is between the bursting of a cannon by hydraulic pressure, or by gun-powder. It is well known that a cannon bursted by hydraulic pressure is perfectly harmless, and the effect of bursting with gun-powder is also known by dreadful experience. In my endeavours to learn the nature and value of high steam, I have been so fortunate as never to have had one explosion, although I have very often had the steam up to a pressure of 100 atmospheres; but I have had more than 100 rends, or burstings. In the Adelaide Gallery where the steam has been up daily to an average of 450 lbs. to the square inch, for more than four years, and where the exhibition of the steam gun has been many times stopped in consequence of the bursting of the generator, yet no report has ever been heard, even by persons within a few feet of the generator. In fact I never knew a brick to be disturbed by the bursting of the generator. About ten years since I was exhibiting the steam gun in a large assemblage, when in the middle of a volley the steam dropped, and the balls refused to leave the barrel. I immediately ran to the furnace and found a rent about

eight inches long and one inch wide in the centre of the boiler, which was three and a half feet long, seven inches diameter, and three-quarters of an inch thick, of wrought copper. I apologised to the company for stopping the experiment as the boiler had burst, they exclaimed that it was impossible, for they had heard no explosion, my answer was, come and see, which many did, when the astonishment manifested, as well as the pleasure expressed, by them, was very great. One gentleman in particular was extremely gratified at being present when the circumstance occurred, he being an owner of steam engines.

The Committee of the Franklin Institute have made experiments upon a subject much pursued by me, namely, to ascertain whether surcharged steam could have its power increased by adding sufficient water to give to it its proper density. In a pamphlet published by me about eight years since, containing observations upon the causes of the explosion of steam boilers, I offered some opinion on this point, which found many believers; the opinions then given were the result of much observation, and of numerous experiments; my experiments, however, to ascertain this fact, were conducted in a very different manner from those of the committee; I built my fire under the bottom of a very strong generator, which would bear a pressure of at least 300 atmospheres. The object of this great strength of the generator was not only to enable me to make my experiment without danger, but to surcharge the steam at a very high temperature, so that there should be no mistake as to the result. The fire was made at the bottom of the generator, about a quarter up the sides, and about as much above the water as below it, observing at the same time to have the fire much more intense above than below the water; but no fire was made *on*, or *near*, the top of the boiler; believing that heat would not descend any more in steam than in water. This fact I have now completely established by a new modification of a boiler which owes much of its success to the property which surcharged steam possesses of transmitting heat; the particulars of this modification cannot be made public until I have obtained my foreign patents therefor. If I had made the fire on the top of the boiler, as the Committee of the Franklin Institute did, in their experiments, I should have made the same mistake, and instead of surcharging the mass of steam, I should only have surcharged a small film next the heated metal, and have left the rest perfectly saturated with water, and quite unfit for receiving a jet of that fluid, which could only serve to lower the temperature and power, which was the case in their experiments.

At the top of my generator the temperature was at least 3000 degrees, as ascertained by an alloy that fused at about that point; the temperature of the steam in contact with the water was about 300 degrees, the mean temperature about 1500 degrees, that together with the heated sides and top of the generator furnished heat enough to make the indicator rise at each stroke of the pump from 50 to 100 atmospheres; and as the steam was constantly blowing off at the safety valve, which was loaded at five atmospheres, the steam would fall to this pressure in about fifteen seconds. The oscillation of the indicator was from four to five times per minute, its rise was instantaneous, but its fall gradual.

The 9th experiment of the committee was "to repeat Perkins' experiments and ascertain whether the repulsion, stated by him to exist between the particles of intensely heated iron and water, be general, and to measure, if possible, the extent of this repulsion with a view to determine the influence which it may have on safety valves." The experiment made with

the perforated thick iron boilers, as far as it went, was very satisfactory. The water standing in a bowl heated up to 800° without passing through the metal, until the metal had lowered its temperature, would be easily seen by any one, and would be an interesting experiment to be repeated in a lecture room.

The committee now proceed to give an account of an unsuccessful attempt to repeat Perkins' experiment referred to in the query. As it does not seem to bear upon the application of the safety valve, they did not deem it necessary to encounter the expense of the apparatus necessary to a further trial. The experiment of Perkins which is more particularly referred to in the query, is that in which an opening having been made in one of his generators, containing intensely heated water in contact with the red hot metal, neither steam nor water escaped, and in which having affixed a pipe and stop-cock to the same vessel, no steam issued through the cock when opened. To repeat this with a view to ascertain, as required in the query, the size of the opening to which such a result would apply, three apertures were made of $\frac{1}{16}$th, $\frac{1}{8}$th and $\frac{1}{4}$th of an inch respectively, in the sides of a wrought iron mercury bottle; these were closed by conical plugs connected with levers, by which the plugs could be withdrawn from the sides of the bottle. The fulcrums of these levers were attached to the wrought iron cylinder already referred to, within which, its axis coinciding with that of the cylinder, the cylindric bottle was placed. An earthen-ware furnace was placed below the bottle and surrounding cylinder, the latter resting upon wrought iron bars supported by the edges of the furnace, and the former supported by a stone placed upon the grate of the furnace. Besides affording a support for the levers, the wrought iron cylinder was introduced to protect the experimenters against injury should the bottle explode in the trials to be made with it. The apparatus having been placed in a quarry pit, adjacent to that in which the cylindric boilers were burst, water was poured into the bottle so as to fill it; the screw plug was next passed into the neck and forced home by lateral blows from a hammer; a fire was now made in the furnace, and the fuel heaped up so as to fill the entire space between the mercury bottle and wrought iron cylinder, and to be about five inches deep above the stopper of the former; a string was attached to the lever connected with the smallest plug and carried up the bank. The fire soon burned briskly, and it was perceived that a small quantity of steam mingled with the feeble smoke and heated air which rose above the apparatus. About twenty minutes after the beginning of the experiments the leak appearing to increase, an incautious attempt was made to stop it, but without success; at this time the bottle was seen to be at a dull red heat. It was thought that but little water had been able to escape in steam through the very minute opening which the imperfect thread of the screw gave, and it was intended to withdraw one of the plugs when a few minutes should have elapsed to give time for the bottle to be heated to complete redness; meanwhile a most violent explosion occurred, the body of the bottle rose in the air, the iron cylinder which served to increase the height of the furnace was throw from its place, the earthen furnace blown to pieces, and the fire scattered far and wide through the woods. After extinguishing the fire, it was found that the iron cylinder, weighing, with the apparatus connected with it, 51$\frac{3}{4}$ lbs. had been thrown four feet from its bed, the plugs which passed into the bottle had been broken short off at the exterior of the bottle, the bottom had been forced into the ground, which was ploughed up by the fragments of the furnace, and completely

wet for a considerable distance around: one of the iron bars supporting the cylinder was thrown a distance of thirty feet and sunk three inches into the ground. The body of the bottle was found thirty yards from its position before the explosion, having penetrated two feet into the ground. The noise of the explosion resembled that of a 12 pounder fully charged. This experiment proved first, that steam, from intensely heated water was able to penetrate an exceedingly small opening; although it proved nothing in regard to an aperture made in a vessel containing water only, it showed an effect produced when there was very little steam in the vessel. It verified the deduction from theory, that but a small part of water highly heated can expand into steam if suddenly relieved from pressure. "It showed that great danger must be incurred in attempts to heat water very highly even in vessels where it has but little room to expand itself, contrary to the opinions entertained by many, and that an attempt to repeat the experiment of Perkins, unless with an apparatus capable of sustaining the most intense pressure, must be attended with great danger." Although it appears that the boiler was first filled with waste water yet under such tremendous pressure the water and steam would be rapidly discharged through the small fissure which was observed to have made its appearance, and as the water lessened its bulk, so as to leave a space between the metal and water, it would be repelled from every part of the boiler except at the fissure, which by its blackness would have shown that the temperature of that spot was very much reduced by the issue of steam and water. If the whole boiler showed a dull redness, as was stated to have been the case by the committee, water could not have been in contact with the boiler, but must have been separated by a thin film of non-conducting material; for had the water been heated to dull redness, a boiler of twenty times the strength of that used, could not have resisted the resulting pressure. I have no doubt that the water was of a much lower temperature than the steam in contact with it, or the metal which was in contact with the steam. It was observed by the committee that just before the explosion the rush of steam increased. This was undoubtedly the cause of the explosion. It has been frequently observed that when the water had been reduced in the boiler so as to allow the heat to enter above the water and surcharge the steam, as well as heating the upper part of the boiler, that if at this time the safety valve be suddenly raised so much so as to take off the steam faster than it is generated, then the pressure would be taken off from the surface of the water so as to allow it to rise up in mist, and take off the extra heat from the surcharged steam, as well as the heat from the top of the boiler, and become completely saturated; at this time an explosion must necessarily take place. Now I conceive that the experimental boiler was in this predicament, and when the fissure had increased sufficiently to take off the pressure from the *cylinder of water* which was in the interior of the boiler, this water would burst into mist and take up the heat in the surcharged steam as well as the heat of the metal, and produce what was witnessed by the experimentalists. Now if the plug had been withdrawn before the explosion took place, steam and water would have rushed from the hole. How could it have been otherwise? was not every part of the heated boiler rendered repellent; the aperture was the only place for its escape; thus it would have been made to appear that the experiment that I published sometime since, was entirely fallacious. Now if the Committee will take the trouble to make the experiment as I made it, or as one was made by Alexander Gordon, Esq., of London, Civil Engineer, they will witness the fact. Mr. Gordon called upon

me sometime since and informed me that he had made an experiment to prove whether the facts, that I had stated respecting the repellent power of heat, were correct. He observed the reason of his not having tried the experiment long before was, because he conceived it entirely contrary to the laws of nature, nor should he have tried it then had he not been called upon by Mr. Ogle, who had been much troubled with the burning out of the bottoms of his tubular boilers. Mr. Gordon stated to Mr. Ogle that if the facts, which I had published sometime since, were correct, it would be easily accounted for, and recommended an experiment, which was determined on. Mr. Ogle's boiler consisted of a group of tubes $3\frac{1}{2}$ feet long, 4 inches diameter, and under one-eighth of an inch thick, standing vertically, arranged very close, and the heat of the fire passing up between them. In one of these tubes a series of cocks which ranged from the bottom to the top, say four or five inches apart, were fixed for observing the changes of the water and steam at different temperatures, these cocks were about a quarter of an inch of interior space. The fire was now gently made under the bottom of the boiler, the steam was soon got up, and was allowed to blow off constantly, as fast as it was generated; these tubes contained about two-thirds water and the other third steam; when the lower cock was first opened water issued as was expected at the temperature; this cock was now closed and the top opened, when the steam rushed out at about 200 lbs. pressure to the inch; the bottom of the tube was now raised to a higher temperature and when the lower cock was opened, steam and water escaped; this cock was then closed and the upper one was opened, and steam and water came out there also. The top was now closed, the fire was urged to a much higher temperature, and the bottom of the boiler showed a dull redness, and when the lower cock was opened nothing but steam issued, this cock being shut, the upper one was opened, and water only issued evidently showing that all the water was at the top of the tube. The fire was now urged to a much greater intensity, and the bottom became red hot, and when the cock was opened nothing was heard, neither was steam seen to issue; but Mr. Gordon thought he smelt hydrogen, which was undoubtedly the case.

If my countrymen would repeat this experiment they would be perfectly satisfied of the correctness of what I have formerly stated. I have been extremely fortunate in meeting, in London, with Professor Hare, of Philadelphia, who is one of the investigating committee of the Institute. Dr. Hare, although one of the committee, has not taken part in any of the experiments, having made up his mind that it was useless to attempt to prevent explosions by any regulations which can be practically enforced. The Doctor's remedy is to prevent mischief when explosions do actually take place. This has always been my aim also, although our two systems are entirely different; mine has been to modify the boiler so as to prevent the possibility of explosions properly so called, bursting being the only result of a pressure of steam too high for the strength of the boiler.

It affords me great pleasure to have had such a man as Dr. Hare to witness these new facts which throw so much light on the science of steam. I shall conclude this paper by requesting my American friends to suspend their opinion of the report of the Franklin Institute (so far as I am concerned) until the return of Dr. Hare, who I hope will take the very machine used in this country to show the fact. JACOB PERKINS.

Physical Science.

Experiments on Solar Light. By JOHN W. DRAPER, M. D. Professor of Chemistry, Hampden Sidney College, Va.

(Continued from Vol. XIX., p. 479.)

43, *Action of vegetable leaves on carbonic acid.* 44, *Analysis of gas evolved by non-luminous heat.* 46, *Analysis of gas under ordinary circumstances.* 47, *Plants evolve nitrogen, and not oxygen gas, in the sunshine.* 52, *Effect of stopping the chemical rays.* 54, *Identity of the primary and resulting volume of the gas.* 58, *Non-oxygenation of phosphorus.* 62, *Decomposition of salts of silver.* 64, *Fundamental trial.* 65, *Discovery of the diffraction of the chemical rays.*

43. ACTION OF VEGETABLE LEAVES ON CARBONIC ACID.—The evolution of gas, depending, therefore, on the rays of heat, we are next led to inquire, whether the chemical rays affect the operation in any manner. To understand this, I exposed a quantity of boiled water, which had been suffered to cool in vacuo, to carbonic acid gas, of which it absorbed a certain amount. A portion of this water was placed in the focus of the brass mirror, 39, and was there acted upon, by the non-luminous rays; its temperature never exceeded 40° Fah. In a short time the pine leaves commenced giving off gas very copiously, and continued to do so; but it was found on trial, that nearly the whole of it was absorbed by lime water, and, that no decomposition had occurred. Therefore, though rays of non-luminous heat are competent to cause the evolving of gas, they are not able to cause decomposition.

44. The record of an analysis, will place this effect in its true light; care being taken that the water should be impregnated with pure carbonic acid gas, and the leaves recent, when a sufficient quantity was evolved, 39 measures were taken, of which caustic potassa absorbed 34. Hydrogen gas being then added, a diminution to the amount of 4 volumes was produced by a platinum ball, the remaining gas was proved to be nitrogen. The composition of this gas was therefore,

Carbonic acid, . .	34.00
Nitrogen, . .	3.67
Oxygen, . .	1.33
	39.00

It is proper to observe that a change very evidently takes place in the structure of the vegetable leaves, their colour becoming of a dirty brown, and all their greenness lost. Whether it is a change of their acting tissue, which hinders decomposition, or whether there is some peculiarity in the constitution of non-luminous heat, which incapacitates it from producing those effects which result from caloric radiating from highly incandescent bodies, I shall proceed to discuss.

45. Let us first consider what is the action of an ordinary unchanged sunbeam, on carbonic acid in solution, and in contact with vegetable matter. A wide distinction is here to be made, between common spring water, such as pump water, and water charged with carbonic acid *only*; the former, contains a compound of oxygen and nitrogen, isometric with protoxide of nitrogen; but the protoxide is a chemical compound, having its two volumes of nitrogen compressed into one, whilst this is a capillary compound, existing with an almost insensible condensation. The process of evolving gas

from spring water, and from carbonated water, is essentially different, the former taking place by an exaltation of temperature, occasioned by the impinging of radiant heat, no kind of decomposition at all going on; but the latter is accompanied by a true decomposition, due to the presence of vegetable matter.

46. This case will be better understood by an analysis of the gas given off from carbonated water. A certain volume of water, had its carbonic acid and all other gaseous impurity expelled, by long continued boiling; it was then rapidly cooled by refrigeratory measures, and impregnated with pure carbonic acid gas; being introduced into a mattrass, (plate I. fig. 16,) with a bunch of pine leaves, the neck of the mattrass dipping under the surface of some mercury contained in a cup, so as to cut off communication with the atmosphere, it was exposed to the sun, the day being very favourable, clear, and hot: 47.50 measures of the gas evolved, were taken; a piece of caustic potassa absorbed 3.50 measures of carbonic acid, the remainder being 44.00 measures; 90 measures of hydrogen were added thereto, making the full volume 134.00 measures; a platinum ball reduced this to 67.00; indicating 22.33 of oxygen, there remaining of nitrogen 21.67. The composition of this gas was, therefore,

Oxygen, . .	22.33
Nitrogen, . .	21.67
Carbonic acid, .	3.50
	47.50

To prove that the remainder here spoken of was really nitrogen, one hundred volumes of the original gas were taken, and the electric spark passed through it; there was no diminution in the volume, nor any carbonic acid gas generated; it could not therefore be carbonic oxide, hydrogen gas, nor any of the carburets of hydrogen; it possessed, moreover, all the negative qualities of nitrogen.

47. But, the solution was composed of carbonic acid and water; great care having been taken to cut off all access from the atmosphere during its preparation, and also during its exposure to the sun, for fear of a capillary interchange of the carbonic acid with the gaseous elements of atmospheric air. None such had occurred. From what source then came the large amount of nitrogen gas evolved? the only elements within the mattrass were carbon, hydrogen, and oxygen, yet here a large amount of nitrogen was found, which could have come from no other source than the pine leaves.

48. This fact, furnishes a clue to the mystery of the decomposition of carbonic acid, by vegetable matter. A compound expressed by the formula $c + 2O$, is exposed under the circumstances detailed; at the completion of the experiment, the constitution of the gaseous elements is expressed by $C + N$. We therefore find, that the vegetable leaves had absorbed one volume, whose composition is expressed by $c + 6$, and had given in exchange one volume whose symbol is N. Or, to speak without symbols, in this experiment the pine leaves absorbed one measure of carbonic oxide, and gave in exchange for it one measure of nitrogen, and the resulting gas contained therefore, half its volume of nitrogen, and half of oxygen, united without sensible condensation.

49. Hitherto it has been supposed by chemists, that when vegetable leaves were placed in carbonated water, they absorbed the carbon and caused the oxygen to be evolved. Vegetable physiologists, botanists, and others, have raised a great many theories upon this fact, which, however, a long

course of experiments assures me are without any foundation. There is no truth in the idea, that plants absorb carbonic acid, and assimilate carbon, and evolve oxygen. On the contrary, they actually evolve nitrogen, and the decomposition of carbonic acid, though remotely brought about by the action of the solar ray, is mainly due to the complex play of affinities of the elementary constituents of the plants.

50. I will here give another example in point, substantiating the same fact under different circumstances. Carbonated water that had been exposed with due care to the sun for two days being provided, 25.75 measures of the resulting gas were taken, and found to contain 1.25 of carbonic acid; for caustic potassa diminished them to 24-50. Next, 31.50 measures of hydrogen were added, making in all 56.00, and a platinum ball being introduced, there remained 7.50 indicating 16.16 volumes of oxygen, and 8.34 of nitrogen, the composition of the gas being therefore,

Oxygen,	16.16
Nitrogen,	8.34
Carbonic acid,	1.25
	25.75

Allowing for unavoidable errors of manipulation, the formula will stand $26 + N$; the leaves therefore had absorbed a compound of oxygen and carbon, whose composition is expressed by the formula, 3. *vap.* $C + O = 1$. that is, condensed into one volume, and had rendered up one volume of nitrogen in return, being of course the same amount; the resulting gas was therefore one-third nitrogen, and two-thirds oxygen, united without sensible condensation.

51. If any further proof was required, that the evolution of nitrogen by the plant, is an essential part of this decomposition, it is furnished by the results of an experiment, in which spun glass was used to replace the pine leaves. This arrangement, though exposed to the sun under the most favourable circumstances, will not evolve any gas, but on passing into it a leaf, no matter how small, decomposition at once commences, because the requisite quantity of nitrogen is given off.

52. A box *a*, *b*, *e*, *e*, of a cubical shape, fig. 1, plate I., and nearly 12 inches in each of its dimensions, had one of its sides taken out, and replaced by a trough *k k* of suitable size, consisting of two glass plates, cemented at a distance of $\frac{1}{4}$ inch from each other. This trough was filled with a solution of bi-chromate of potassa; one of the sides of the box was hung on hinges *e e*, as a door, for the sake of obtaining access to the interior. Within this little chamber, a mattrass filled with carbonated water, and enclosing a bunch of pine leaves, its neck dipping beneath the surface of some water in a cup, was shut up, and exposed to the sun's rays, which, passing through the trough, impinged upon it. In a short time, air bubbles were copiously given off, and when a sufficient quantity was obtained for analysis, its constitution was determined. The following is selected from a number of analyses, being probably the most correct, and very nearly the mean.

Carbonic acid,	16.00
Oxygen,	8.16
Nitrogen,	4.84
	29.00

We here remark the existence of a far larger proportion of carbonic acid, but the relative proportion of the oxygen and nitrogen is still observed with

tolerable accuracy, the deviation may be satisfactorily referred to disturbing causes. The greater amount of carbonic acid, as compared with sections 46 and 50, may likewise be due to the higher temperature of the arrangement when shut up in a close box, where currents of air, or other cooling agents, could not have free access to it. Or, it may hereafter be found, that there are chemical rays of *different colours*, as it were; or, more strictly of different refrangibility, and absorbability, and that those which find a passage through bi-chromate of potassa, can cause the decomposition of carbonic acid, though they cannot blacken chloride of silver. The doctrine that chemical rays are nothing more than undulations of an elastic medium, whose waves vary in breadth, I shall endeavour to support; each of these kind of waves, being competent to bring about changes peculiar to itself; or, adopting another hypothesis, that they are particles whose axes perform certain oscillatory movements. Not in this place, however, to anticipate what I have to offer on these matters; I shall continue to use the term *chemical rays*, as expressing those which blacken chloride of silver; and these, I say, are not engaged in the decomposition of carbonic acid.

53. From the first observations made on the decomposition of carbonic acid by Priestley, this subject has afforded much scope for chemical speculation. Count Rumford examined it successfully, but wanting means of accurate gaseous analysis, and above all not understanding the doctrine and laws of interchange through tissues, his conclusions are devoid of that degree of precision, which the advance of chemistry, in all its departments, enables us to attain. The conclusion to which these earlier philosophers came was, that plants had the power of absorbing carbonic acid, and rendering oxygen in return, by elaboration from their vessels; and this they regarded as the great means employed by nature to maintain the integrity of the composition of the atmosphere. A similar view has been taken of this subject by almost every philosopher, who has since examined it. Professor Burnet, to accommodate the theory to the observed facts, infers that plants exercise two functions, the one of breathing, the other of digestion, the latter only occurring during the stimulant action of the sunshine. This phenomenon is, however, unquestionably, one depending on the exalted capillary action of a tissue when radiant matter impinges on it; and the evolution of nitrogen or of some other gaseous or vaporous matter is, therefore, an essential part of the process.

54. The calculations of analyses made in the foregoing sections, involve the principle, that the volume of gas which remains after action is complete, is exactly the same as the volume of carbonic acid first operated on. The best method of proving this, is to take a tube, the diameter of which may be half an inch or upwards, which is graduated to inches and decimal parts. Fill it with water, from which all gaseous matter has been expelled by long continued boiling, place a few vegetable leaves in it, carefully removing any bubbles of air which may be attached to them, invert the tube in a vessel of water, and pass into it, as quickly as possible, a measured quantity of pure carbonic acid, and transfer it to a mercurial trough. This arrangement is seen in fig. 5, plate I. Conduct the experiment, first, in a cool dark place, absorption will rapidly go on, and in a short time all, or the greater part of, the carbonic acid will disappear, a column of mercury *e e*, rising in the tube to replace the gas. It is to be remarked, that it is not always easy to procure the entire absorption of all the gas, a little bubble remaining in the upper part of the tube, containing the impurities that may have existed in the gas, and also any remains of the carbonic acid, for the

amount absorbed depends upon several circumstances, as to the relative proportion of the volume of gas to the volume of water, the height of the mercurial column suspended in the tube, the temperature of the arrangement, &c. Then, on exposure to the solar rays, gas is copiously given off, the quantity continually decreasing until any further exposure ceases to evolve any more. On making the usual corrections for temperature and pressure, the aggregate of evolved gas will be found precisely the same as the volume first operated on.

55. Precisely I say, but this is with certain restrictions. Sometimes the volume is increased by an amount varying from .10 downwards; due chiefly to a certain amount of gas given off from the leaves extraneously; and partly to the capillary action of the whole system upon the elements of atmospheric air, which are transferred by slow degrees to the water operated upon, should there be a film of that fluid between the mercury and the sides of the glass tube; but, by making allowance for these disturbing actions, the proportion of equality will be found to be rigidly observed by the absorbed and the evolved gases.

56. We find, therefore, that the evolution and decomposition of carbonic acid by the solar ray, are due to that part of it exciting heat; that the chemical ray has no direct agency in the matter; it may bring about changes which to a certain extent complicate the phenomenon, but that it does not produce the abstraction of any compound of oxygen and carbon, from carbonic acid. Apart from the agencies exercised by the elements of the plant, agencies which are unquestionably of the least importance, the decomposition is remotely brought about by the action of radiant matter. But non-luminous heat, though capable of evolving gas, produces no change of its constitution; (section 44,) shall we then suppose that there is a difference in point of quality, between the heat given off by the bodies below ignition, and the heat of incandescent matter? Or, does the light itself aid decomposition. An experiment may be made, which appears to me, to bear directly on the answer which should be given to this query. Let a beam of light, fig. 3, plate I. two inches in diameter, pass through an aperture in the shutter A B, and fall upon any medium, *c d*, which should absorb a certain number of the rays of heat, as bichromate of potassa, which may be so diluted, as to absorb exactly 50 rays, out of every 100. Having, by means of a good thermometer, *g*, measured this, let the beam of light pass through a second trough *e f*, containing the same solution of the same strength, and its temperature again be taken, it will appear that the ray instead of losing half its heat, will contain nearly all of it, or in other words the second trough exerts no action on the passing beam. In an experiment, tried after the manner here indicated, the thermometer having shown a loss of 50 rays by the action of the first trough, fell only to 47, or gave a loss of 3 rays only, as the action of the second trough, an action to be referred, undoubtedly, to a degree of turbidness, which does exist to a small extent, in the clearest solutions; and also to the reflective and scattering action of the surfaces of the troughs. Now, the very same thing takes place in the case of light. A beam that has passed through a green or any other coloured glass, loses a large amount of its intensity, but if it pass through a second plate of the same tint, the second loss is entirely disproportionate to the former, and the reason of this is very apparent, for if the second plate had been of a different colour, the ray might have been much more affected, or even entirely extinguished. Delaroche made an identical observation in the case of non-luminous heat, for he proved, that

a plate of glass obstructed a large portion of the rays falling on it, but that a second plate allowed these rays to pass with far less loss. Now these experiments would lead us to conclude, that there are essential differences in radiant heat, analogous to the differences in light. The rays of heat given off by a cannister of hot water may be, to use an expressive solecism, *violet* heat, and a piece of transparent glass may be able to transmit *green* heat *only;* hence, in using two plates, the absorptive action of the first has the largest share in producing the phenomenon, the second transmitting nearly all which passed the first, an action identical with that of coloured glass on light. Bodies, as their temperature rises, emit more and more rays capable of passing through glass, simply because they become of that class over which the medium does not exercise an absorptive power.

57. The general conclusion which we are to draw from these researches is, that the decomposition of carbonic acid gas by vegetable matter, is a very complex phenomenon, due to the combined action of three forces, 1st the decomposing action of a tissue, 2nd to the impinging of radiant heat, 3d to chemical affinity, it being probable that any of these alone would be incompetent to produce this result. And, in the case of gas, such as oxygen being evolved from spring water, we are to refer the change to the ready decomposition of capillary compounds, compounds essentially distinct from chemical, and which can suffer decomposition by the force of capillary action. The colorific and the chemical rays have no influence in this latter case.

58. Non-oxygenation of Phosphorus. It is stated in the books, that Ritter in making observations on the slow combustion of phosphorus at common temperature, found that it emitted white fumes in the movable red ray of the solar spectrum, but in the movable violet, phosphorus in a state of oxygenation, was instantly extinguished. As a similar action is alleged to take place when the sun's rays shine on ignited carbon, it becomes desirable to understand the mode of action,—the original experiment of Ritter, was therefore repeated with a view to ascertain its accuracy. A cylinder of phosphorus, *a b* fig. 6. plate I., an inch long, and about one-sixth of an inch in diameter, was shielded from the action of aerial currents, by a glass jar. In front of the jar, an equiangular prism of flint glass was placed, so that the rays of a decomposed beam of light coming in through the shutter *c d*, could successively be thrown on the phosphorus which was placed horizontally in the jar; the beam of light also came nearly horizontally into the room, reflected by the arrangement already described. Situated thus, by turning the prism on its axis, any ray could be made to cover the phosphorus, the temperature in the shade being 72° Fah., a fine sheet of metaphosphoric acid, mingled with vapour of phosphorus, so thin as to be almost imperceptible except in certain positions, was observed to be rising from the cylinder, sometimes it would form a fine nebulous cloud, which hung for a moment on the phosphorus, and then rise gracefully in curled wreaths. The extreme mobility of this cloud was remarkable, even the warmth of the observer, by causing currents within the jar, would affect it, if the hand approached as at A, fig. 7, plate I., the phosphoric vapour came to the side of the vessel as it were to meet it, and then rebounded and circulated along the top of the jar. The size, position, and shape of this cloud when enveloped in the red light of the prism were deliberately marked, its motions were merely more capricious than when in the shade. And now, by turning the prism, the extreme *violet* ray was

brought upon it, but neither did its motion, or magnitude, or figure, appear in any wise changed.

59. The impression conveyed by Ritter's experiment is, that the chemical rays possess the faculty of hindering oxygenation. The negative conclusion here arrived at, might be due to local circumstances, and be referred to the action of the prism, as to its composition, to the state of the atmosphere, &c.; but no better success attended a variety of trials made on different days, and with prisms of crown glass, turpentine, and water. Trial was therefore made of absorbing media, a beam of light being made to pass at one time through a solution of sulphate of copper and ammonia, and at another through bichromate of potassa, but the condition of the phosphorescent cloud, was found to be too rough an estimate of the real action. A cylinder of glass A B, fig. 13, plate I., .75 inch in diameter and 3 inches long, was therefore fitted at its upper end with a stop-cock *a*, its lower extremity was closed air tight with a cork, through which an inverted syphon *b b* passed, each of its limbs being four inches long and the bore being $\frac{1}{6}$th of an inch; the outer limb was fitted with a scale. After having opened the cock *a*, a stick of dry phosphorus *e* was suspended in the cylinder, which was made very clean and dry, and the syphon being filled with water, was firmly seated in its place and the cock closed. Now as the phosphorus oxydized, the metaphosphoric acid was removed by the water present, and the level falling in the laternal limb, indicated what quantity of oxygen was consumed, and therefore the rate of combustion of the phosphorus. This was expected to give a more accurate estimate of any changes occurring in the phenomenon, and was accordingly applied to detect them.

60. A column of different coloured light, being made to pass at different times through the cylinder, so as to impinge on the phosphorus, attempts were made to ascertain the rate of combustion for each, as also for the white light of the sun, and in the shade. In each insulated experiment, the fluid in the gauge sunk with great regularity, more rapidly at first, but then more slowly, but the same regularity was not observed in different trials. At one time the phosphorus would consume with more than double the rapidity that it did at another, though to all appearance under identical circumstances. If the slow combustion of phosphorus be at all affected by the action of solar light, it is certainly not to that extent which Ritter supposed. So far from extinguishing, the violet rays do not exert any control over it, or if they do, it is to so small an extent that the most delicate arrangements fail to detect it.

61. It is possible however that atmospheric temperature may exert an influence on the result. During the trials here made, a thermometer in the shade has ranged from 70° Fah. to 82° Fah. At these points, the affinity of the combustible material for oxygen may be so exalted, that the action of any weaker force becomes masked. It is not stated what were the temperatures at which the alleged results were obtained. But it is most probable that the presence of extraneous matter has been the cause of all these variations. It is well known that certain compounds of hydrogen and carbon, in extremely minute quantity, will entirely put a stop to the oxidation of phosphorus, and during the course of these trials, I have had abundant reason to notice errors arising from this cause. By simply wiping out the cylinder with a linen cloth, which contained an almost imperceptible trace of spirits of turpentine, an erroneous result like that of Ritter was at once obtained.

62. Decomposition of the Salts of Silver. Several of the salts of silver, undergo a remarkable change when exposed to the rays of light. The bromide, the chloride, and the nitrate, being very good examples; these, which are all white, become of a dark colour approaching almost to black, when exposed to the violet rays; it is stated that the bromide is most readily affected, yielding a brownish black colour.

63. If a piece of paper be soaked in a solution of nitrate of silver, and then dipped into a solution of bromide of potassium, it affords a very advantageous means of facilitating these experiments. The chloride may occasionally be substituted for the bromide of silver.

64. A beam of light, fig. 2 *a a*, entered a dark chamber horizontally, and was obstructed in its course by a plane metallic screen *b*, having a hole half an inch in diameter in it. The beam *c*, which passed through this aperture, fell upon a flint glass equiangular prism *d*, and was decomposed by it, the spectrum *e f* being received on the table, this spectrum was about three inches long. And now a piece of paper, imbued with bromide of silver, was placed to receive it, with the intention of ascertaining how far the discoloration would extend. In the course of five minutes, a very marked change had taken place, and on examination it was found that the deepest tint had been occasioned where the violet blended with the indigo rays; beyond this, even in the dark space beyond the spectrum, there was a stain, as also as far in the spectrum as where the green light merged into the yellow, an effect represented in fig. 14, *a a*, *b b*, being the spectrum, during this experiment the spectrum was kept stationary. Again, a column of light three inches in diameter, converging from a convex lens *a a* fig. 15, was intercepted by a screen of pasteboard *b b*, which had a circular aperture in it, half an inch in diameter, this screen was placed at such a distance from the focus, that the circular section of the cone of light was half an inch in diameter, and therefore passed exactly through the aperture; a piece of the prepared bromide paper, was then fastened on the back of the screen, so as to receive the condensed rays which passed the aperture. In a few moments a black spot appeared about the central parts of the paper, and at the end of the experiment there was an intensely black circle surrounded by a brown ring-like penumbra, as in fig. 8; the diameter of the black spot being three-quarters less than that of the aperture through which the light passed.

65. Diffraction of Chemical rays. Under certain circumstances, two ærial vibrations, each of which, if separately striking the organs of hearing, would produce a musical sound, may so interfere with each other as to produce an unmelodious rattling, or even silence. Also, two rays of light whose paths bear a certain relation to one another, instead of increasing each other's intensity, may have a directly opposite effect, and neutralizing each other, produce darkness. It becomes therefore a question, not only of mere curiosity, but one whose bearings are important, to find if the chemical rays emitted from the sun, when placed under similar circumstances, exhibit similar phenomena. For then analogy would lead us to know that it is possible for two rays of heat to be so situated with regard to one another, that instead of exalting the temperature of the body on which they fell, to lower it, or in other words to produce actual *cold*.

66. In my early trials for the solution of this question I met with many disappointments, but at last I fell upon an arrangement which yielded positive information. It is however an experiment requiring careful manipulation. A horizontal beam of light being projected into a room by the apparatus

heretofore so often referred to, at extremity *e e*, fig. 12, plate I. of the brass tube, a double convex lens of short focus was screwed, this brought the rays to a point at a distance of three-quarters of an inch from the lens, here they were obstructed by a metallic screen *b b*, having a round hole *c*, one-eighth of an inch in diameter, perforated in it. This screen, revolved about a vertical axis on a piliar *d*, so that it could be brought to any angle with the incident rays. The rays passing through the round hole *c*, were received on a white screen *g g*, at a distance of six inches. When the screen *b b* received the incident rays perpendicularly to its surface, then, of course, the image thrown on the screen *g g* was circular, but if the screen *b b* was made to receive these rays at an acute angle, then the image was lenticular. Under the last condition, the phenomena of diffraction is represented in fig. 9, plate I. where *a a* is the screen, *b b* the lenticular image cast on it, which is of bright white light, except at its central part *c*, where there is a dark image produced by the interference of the passing rays.

67. If in such an arrangement the chemical rays do not interfere with each other so as to neutralize their effects, chemical effects should be produced in every part of the image, even including its central part *c*; but if, on the other hand, these rays are obedient to the same laws as the rays of light, then in the central parts of the image no chemical effects should ensue; the problem is therefore reduced to the finding how any compound, changeable by these rays, will comport itself on the central and peripheral parts of such an image.

68. In place of the screen *g g*, a substitute was used consisting of two thin plates of mica, with a lamina of bromide of silver included between them, these were mounted in a little ivory frame *a b c d*, fig. 4, just in the manner that objects are usually mounted for the use of the microscope, and the lenticular image cast upon the bromide. After an exposure of five minutes, during which care was taken to keep the sun's place perfectly immovable, and also to avoid all local tremor, which might make the image traverse on the bromide, the result was very apparent, being as represented in fig. 11, of the natural size, the peripheral parts being of a deep brown, and the centre yellowish white. Viewed through a lens, the boundary line was not so sharp and distinct, but seemed to merge by insensible gradation into the unaffected part, as in fig. 10. The conclusion to be drawn from this result, possesses no common interest. For the same cogent reasoning, which applies in the proof, that light consists of undulations of an elastic medium, applies here also.

(TO BE CONTINUED.)

Progress of Practical and Theoretical Mechanics and Chemistry.

On the application of the Hot Blast, in the Manufacture of Cast-iron. By THOMAS CLARK, M. D., *Professor of Chemistry in Marischal College, Aberdeen.*

(Read 16th March, 1835.)

Among persons interesting themselves in the progress of British manufactures, it can scarce fail to be known, that Mr. NEILSON of Glasgow, manager of the Gas Works in that city, has taken out a patent for an important improvement in the working of such furnaces as, in the language of the patent, "are supplied with air by means of bellows, or other blowing

Fig. 1.

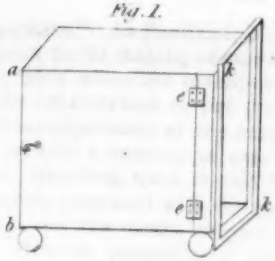


Fig. 2.

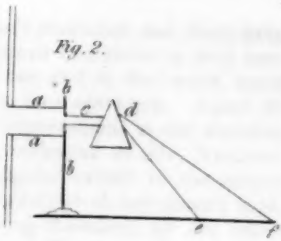


Fig. 5.

Fig. 3.

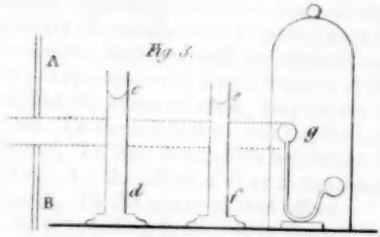


Fig. 4.

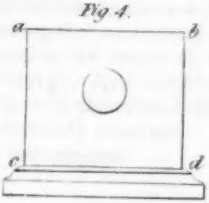


Fig. 6.

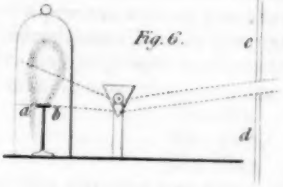


Fig. 7.

Fig. 8.

Fig. 12.

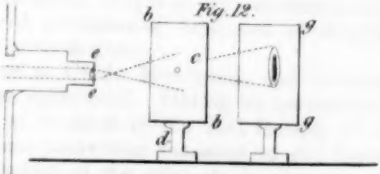


Fig. 9.

Fig. 13.

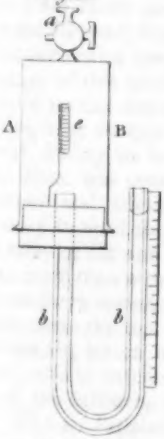


Fig. 14.

Fig. 10.

Fig. 15.

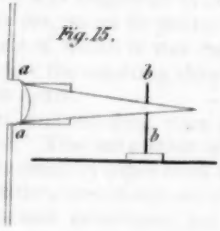


Fig. 11.

Fig. 16.

apparatus." In Scotland, Mr. NEILSON's invention has been extensively applied to the making of cast-iron, insomuch that there is only one Scotch iron work where the invention is not in use, and in that work apparatus is under construction to put the invention into operation. Apart from the obvious importance of any considerable improvement in the manufacture of so valuable a product as cast-iron, the invention of Mr. NEILSON would merit attention, were it only for the singular extent of the improvement effected, compared with the apparent simplicity—I had almost said inadequacy—of the means employed. Having therefore, by the liberality of Mr. DUNLOP, proprietor of the Clyde Iron Works, where Mr. NEILSON's invention was first put into operation, obtained full and free access to all information regarding results of trials of the invention in those works, on the large scale of manufacture, I cannot help thinking that an authentic notice of these results, together with an attempt to explain the cause of them, will prove acceptable to the Royal Society of Edinburgh. And that these results, as well as the cause of them, may be set forth with clearness I shall advert,

First, To the process of making iron, as formerly practised:

Second, To Mr. NEILSON's alteration on that process:

Third, To the effect of that alteration:

Fourth, To the cause of that effect.

I. In proceeding to advert to the process of making cast-iron, as formerly practised, it cannot here be necessary to enter into much detail in explanation of a process so long practised and extensively known, as this has been; nor, indeed, shall I enter into details farther than, to the general scientific reader, may be proper to elucidate Mr. NEILSON's invention.

In making cast-iron, then, the materials made use were three,

The Ore,
The Fuel,
The Flux.

The Ore was clay iron-stone, that is to say, carbonate of iron, mixed, in variable proportions, with carbonate of lime, and of magnesia, as well as with aluminous and silicious matter.

The Fuel made use of at Clyde Iron-works, and in Scotland generally was coke derived from splint-coal. During its conversion into coke, this coal underwent a loss of 55 parts in the 100, leaving 45 of coke. The advantage of this previous conversion consisted in the higher temperature produced by the combustion of the coke, in consequence of none of the resulting heat disappearing in the latent form, in the vapours arising from the coal, during its conversion into coke.

The Flux was common limestone, which was employed to act upon the aluminous and silicious impurities of the ore, so as to produce a mixture more easy to melt than any of the materials of which it was made up, just as an alloy of tin and lead serves as a solder, the resulting alloy being more easy to melt than either the lead or the tin apart.

These three materials—the ore, the fuel, and the flux, were put into the furnace, near the top, in a state of mixture. The only other material supplied was air, which was driven into the furnace by pipes from blowing apparatus, and it entered the furnace by nozzles, sometimes on two opposite sides of the furnace, sometimes on three, and sometimes, but rarely, on four. The air supplied in this manner entered near the bottom of the furnace, at about 40 feet from the top, where the solid materials were put in. The furnace, in shape, consisted, at the middle part, of the frustums of two cones having a horizontal base common to both, and the other and smaller

ends of each prolonged into cylinders, which constituted the top and bottom of the furnace, as may be well enough conceived from the sectional sketch on the margin.

The whole of the materials put into the furnace, resolved themselves into gaseous products, and into liquid products. The gaseous products, escaping invisible at the top, included all the carbonaceous matter of the coke, probably in the form of carbonic acid, except only the small portion of carbon retained by the cast-iron. The liquid products were collected in the cylindrical reservoir, constituting the bottom of the furnace, and there divided themselves into two portions, the lower and heavier being the melted cast-iron, and the upper and lighter being the melted slag, resulting from the action of the fixed portion of the flux upon the fixed impurities of the fuel and of the ore.

II. Thus much being understood in regard to the process of making cast-iron, as formerly practised, we are now prepared for the statement of Mr. Neilson's improvement.

This improvement consists essentially in heating the air in its passage from the blowing apparatus to the furnace. The heating has hitherto been effected by making the air pass through cast-iron vessels, kept at a red heat. In the specification of the patent, Mr. Neilson states, that no particular form of heating apparatus is essential to obtaining the beneficial effect of his invention; and, out of many forms that have been tried, experience does not seem to have yet decided which is best. At Clyde Iron-works, the most beneficial of the results that I shall have occasion to state, were obtained by the obvious expedient of keeping red-hot the cast-iron cylindrical pipes conveying the air from the blowing apparatus to the furnace.

III. Such being the simple nature of Mr. Neilson's invention, I now proceed to state the effects of its application.

During the first six months of the year 1829, when all the cast-iron in Clyde Iron-works was made by means of the cold blast, a single ton of cast iron required for fuel to reduce it 8 tons $1\frac{1}{4}$ cwt. of coal converted into coke. During the first six months of the following year, while the air was heated to near 300° Fahr., one ton of cast-iron required 5 tons $3\frac{1}{4}$ cwt. of coal, converted into coke. The saving amounts to 2 tons 18 cwt. on the making of one ton of cast-iron; but from that saving comes to be deducted the coals used in heating the air, which were nearly 8 cwt. The nett saving thus was $2\frac{1}{2}$ tons of coal on a single ton of cast-iron. But during that year, 1830, the air was heated no higher than 300° Fahr. The great success, however, of those trials, encouraged Mr. Dunlop, and other iron masters, to try the effect of a still higher temperature. Nor were their expectations disappointed. The saving of coal was greatly increased, insomuch that, about the beginning of 1831, Mr. Dixon, proprietor of Calder Iron-Works, felt himself encouraged to attempt the substitution of raw coal for the coke before in use. Proceeding on the ascertained advantages of the hot blast, the attempt was entirely successful; and, since that period, the use of raw coal has extended so far as to be adopted in the majority of the Scotch iron-works.

The temperature of the air under blast had now been raised so as to melt lead, and sometimes zinc, and therefore was above 600° Fahr., instead of being only 300°, as in the year 1830.

The furnace had now become so much elevated in temperature, as to require, around the nozzle of the blow-pipes, a precaution borrowed from the finery furnaces, wherein cast-iron is converted into malleable, but seldom, or never, employed where cast-iron is made by means of the cold blast. What is called the *Tweer* is the opening in the furnace to admit the nozzle of the blow pipe. This opening is of a round, funnel-shape, tapering inwards, and used always to have a cast-iron lining, to protect the other building materials, and to afford them support. This cast-iron lining was just a tapering tube nearly of the shape of the blowpipe, but large enough to admit it freely. Now, under the changes I have been describing, the temperature of the furnace became so hot near the nozzles, as to risk the melting of the cast-iron lining, which, being essential to the *tweer*, is itself commonly called by that name. To prevent such an accident, an old invention called the *water-tweer* was made available. The peculiarity of this tweer consists in the cast-iron lining already described, being cast hollow instead of solid, so as to contain water within; and water is kept there, continually changing as it heats, by means of one pipe to admit the water cold, and another to let the water escape when heated.*

During the first six months of the year 1833, when all these changes had been fully brought into operation, one ton of cast-iron was made by means of 2 tons 5¼ cwt. of coal, which had not previously to be converted into coke. Adding to this 8 cwt. of coal for heating, we have 2 tons 13¼ cwts. of coal required to make a ton of iron; whereas, in 1829, when the cold blast was in operation, 8 tons 1¼ cwts. of coal had to be used. This being almost exactly three times as much, we have, from the change of the cold blast to the hot, combined with the use of coal instead of coke, *three times as much iron made from any given weight of splint coal.*

During the three successive periods that have been specified, the same blowing apparatus was in use, and not the least remarkable effect of Mr. Neilson's invention, has been the increase in efficacy of a given quantity of air in the production of iron. The furnaces at Clyde iron-works, which were at first three, have been increased to four, and the blast machinery being still the same, the following were the successive weekly products of iron during the periods already named, and the successive weekly consumption of fuel put into the furnace, apart from what was used in heating the blast:

	tons	tons	tons
In 1829, from 3 furnaces,	111 Iron	from 403 Coke,	from 888 Coal.
In 1830, from 3 furnaces,	162 Iron	from 376 Coke,	from 836 Coal.
In 1833, from 4 furnaces,	245 Iron		from 554 Coal.

Comparing the products of 1829 with the products of 1833, it will be observed that the blast, in consequence of being heated, has reduced more than double the quantity of iron. The fuel consumed in these two periods we cannot compare, since, in the former, coke was burned, and in the latter, coal. But on comparing the consumption of coke in the years 1829 and 1830, we find that although the product of iron in the latter period was increased, yet the consumption of coke was rather diminished. Hence the increased efficacy of the blast appears to be not greater than was to be expected, from the diminished fuel that had become necessary to smelt a given quantity of iron.

* An incidental advantage attended the adoption of the water-tweers, inasmuch as these made it practicable to lute up the space between the blow-pipe nozzle and the tweers, and thus prevent the loss of some air that formerly escaped by that space, and kept up a bellowing hiss, which, happily, is now no longer heard.

On the whole, then, the application of the hot blast has caused the same fuel to reduce three times as much iron as before, and the same blast twice as much as before.

The proportion of the flux required to reduce a given weight of the ore, has also been diminished. The amount of this diminution, and other particulars, interesting to practical persons, will appear on reference to a tabular statement supplied by Mr. DUNLOP, and printed as an appendix to this paper. Not further to dwell on such details, I proceed to the last division of this paper, which is,

IV. To attempt an explanation of the foregoing extraordinary results. Subsidiary to this attempt, it is necessary to discriminate between the quantity of fuel consumed and the temperature produced. For instance, we may conceive a stove to be kept at the temperature of 500° Fahrenheit, and lead, to be put into such a stove for the purpose of being melted. Then, since the melting point of lead is more than 100° higher, it is evident that whatever fuel might be consumed in keeping that stove at the temperature of 500°, the fuel is all consumed to no purpose, so far as regards the melting of lead, in consequence of deficiency in the temperature. In the manufacture of cast-iron likewise, experience has taught us, that a certain temperature is required in order to work the furnace favourably, and all the fuel consumed so as to produce any lower degree of temperature, is fuel consumed in vain. And how the hot blast serves to increase the temperature of a blast furnace, will appear on adverting to the relative weights of the solid and of the gaseous materials made use of in the reduction of iron.

As nearly as may be, a furnace, as wrought at Clyde iron-works in 1833, had two tons of solid materials an hour put in at the top, and this supply of two tons an hour was continued for twenty-three hours a day, one half hour every morning, and another every evening, being consumed in letting off the iron made. But the gaseous material, the hot air, what might be the weight of it? It can easily be ascertained thus: I find, by comparing the quantities of air consumed at Clyde iron-works, and at Calder iron-works, that one furnace requires of hot air from 2500 to 3000 cubical feet in a minute. I shall here assume 2867 cubical feet to be the quantity: a number that I adopt for the sake of simplicity, inasmuch as, calculated at an avoirdupois ounce and a quantity which is the weight of a cubical foot of air at 50° Fahrenheit, these feet correspond precisely with 2 cwt. of air a minute, or *six tons an hour*. Two tons of solid material an hour, put in at the top of the furnace, can scarce hurtfully affect the temperature of the furnace, at least in the hottest part of it, which must be far down, and where the iron, besides being reduced to the state of metal, is melted, and the slag, too, produced. When the fuel put in at the top is coal, I have no doubt that, before it comes to this far down part of the furnace, the place of its useful activity, the coal has been entirely coked; so that, in regard to the fuel, the new process differs from the old much more in appearance than in essence and reality. But if two tons of solid material an hour, put in at the top, are not likely to affect the temperature of the hottest part of the furnace, can we say the same of six tons of air an hour, forced in at the bottom near the hottest part? The air supplied is intended, no doubt, and answers, to support the combustion; but this beneficial effect is, in the case of the cold blast, incidentally counteracted by the cooling power of six tons of air an hour, or two cwt. a minute, which, when forced in at the ordinary temperature of the air, cannot be conceived of otherwise than as a prodigious refrigeratory passing through the hottest part of the furnace, and repressing

its temperature. The expedient of previously heating the blast obviously removes this refrigeratory, leaving the air to act in promoting combustion, without robbing the combustion of a portion of the heat it produces.

Such, I conceive, is the palpable, the adequate, and very simple, explanation of the extraordinary advantages derived in the manufacture of cast-iron, from heating the air in its passage from the blowing apparatus to the furnace.

Marischal College, Aberdeen, Jan. 10, 1835.

APPENDIX.

TABLE *Showing the Weight of Cast-Iron produced, and the average Weight of Coals made use of, in producing a Ton of Cast Iron at Clyde Iron-works, during the years* 1829, 1830, *and* 1833, *the blowing engine being the same.*

Coke and cold air.			Coke and heated air.			Coal and heated air.		
1829	Weekly product of cast-iron by three furnaces.	Average of coals used to 1 ton of cast-iron.	1830	Weekly product of cast-iron by three furnaces.	Average of coals used to 1 ton of cast-iron.	1833	Weekly product of cast-iron by four furnaces.	Average of coals used to 1 ton of cast-iron.
	ton cwt qr	T cwt q		ton cwt qr	T cwt q		ton cwt qr	T cwt q
Jan. 7	137.18.2	8.12.1	Jan. 6	176.10.2	5. 2.2	Jan. 9	375. 8.0	2.12.3
14	148. 2.0	6. 9.2	13	181.12.2	5. 0·2	16	267.18.0	2. 4.0
21	148. 8.2	6.11.3	20	172. 5.2	5. 0.2	23	270. 7.2	2. 3.1
28	138. 9.2	7. 0.2	27	178. 7.0	4.19.0	30	250. 9.0	2. 4.0
Feb. 4	125.13.0	7.12.1	Feb. 3	164. 8.0	5. 4.0	Feb. 6	265. 3.2	2. 1.0
11	136.19.0	7·13.1	10	172.12.0	5. 4.0	13	202.10.0	2. 4.3
18	130.16.2	7·11.3	17	163. 9.0	5. 9.0	20	257. 1.0	2. 4.3
25	105.12.2	7.10.0	24	170. 1.0	5. 3.0	27	264. 0.0	2. 5.1
Mar. 4	101. 8.1	7·17.2	Mar. 3	154.19.0	5.10.3	Mar. 6	234.13.0	2. 5.2
11	111. 2.0	8. 2.2	10	154.16.0	5. 9.2	13	238. 7.2	2. 7.1
18	114.10.0	7. 6.2	17	151. 8.2	5. 9.3	20	205.13.0	2.10.2
25	110.14.0	8. 8.1	24	163.17.0	5. 5.1	27	217.14.0	2. 2.3
Ap. 1	111. 4.0	8. 7.2	31	163. 8.2	5.11.0	Ap. 3	220. 7.0	2.14.2
8	107. 7.0	8. 3.0	Ap. 7	147.10.0	5. 7.0	10	280. 9.2	2. 0.3
15	91.12.2	8.15.0	14	154. 9.2	5. 2.0	17	304. 7.0	1.17.3
22	85.13.0	9.13.0	21	163. 4.0	4.19.0	24	248.12.2	2. 3.0
29	91.14.2	9. 6.2	28	148.12.2	5. 4.0	May 1	245. 7.2	2. 6.0
May 6	92. 7.2	8. 8.2	May 5	162.10.2	5. 2.2	8	200.17.0	2. 8.0
13	94. 6.0	9. 2.1	12	149.13.0	5. 3.2	15	246. 4.2	2. 5.3
July 8	88. 4.2	8.16.3	19	162. 4 0	5. 5.0	22	219. 1.2	2. 6.0
15	91.13.0	8. 5.0	26	165. 7.2	4.18.3	29	231. 2.0	2. 8.0
22	97. 2.0	8. 2.1	June 2	160. 4.0	5. 2.2	June 5	335.16.0	2. 6 2
29	104.15.2	7.10.2	9	157.17.0	5. 1.0	12	232.10.0	2. 7.1
Aug. 5	106.17.2	7. 7.2	16	164. 0 0	4.17.3	19	271. 1.2	2. 1 0
12	93. 1.0	8. 6.0	23	149. 3.0	4.18.0	26	262. 3.2	2. 3.1
19	113. 7.0	8.18.2	30	162.16.2	4.16.3	Jw. 30	122.16.0	2. 5.1
	2878.18.0	209.19.0		4215. 6.0	134. 6.2		6370. 3.0	58.18.3
Aver.	110.14.2	8. 1.1		162. 2.2	5. 3.1		245. 0.0	2. 5.1

The blowing-engine has a steam cylinder of 40 inches diameter, and blowing cylinder of 8 feet deep and 80 inches diameter, and goes 18 strokes a minute. The whole power of the engine was exerted in blowing the three furnaces, as well as in blowing the four, and in both cases there were two tweers of 3 inches diameter to each furnace. The pressure of the blast was 2½ lbs. to the square inch. The fourth furnace was put into operation after the water tweers were introduced, and the open spaces around the blowpipes were closed up by luting. The engine then went less than 18

strokes a minute, in consequence of the too great resistance of the materials contained in the three furnaces to the blast in its passage upwards.

Materials constituting a charge.

		cwt.	qrs.	lbs.
1829,	Coke,	5	0	0
	Roasted Ironstone,	3	1	14
	Limestone,	0	3	16
1830,	Coke,	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone,	1	1	16
1833,	Coal,	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone,	1	0	0

Trans. Roy. Soc. Edin.

An Experimental Inquiry into the Modes of Warming and Ventilating Apartments. By Andrew Ure, M. D., F. R. S.

The author having been consulted by the Directors of the Customs Fund of Life Assurance, on the mode of ventilating the Long Room in the Custom House, and deeming the subject one of great public interest, was induced to lay the result of his observations and experimental inquiries before the Royal Society. In this room, about two hundred persons are busily engaged in transacting the business of the Institution. All these persons are found to suffer more or less from ailments of the same general character, the leading symptoms of which are a sense of fulness and tension in the head, flushing of the face, throbbing of the temples, giddiness, and occasional confusion of ideas, depriving them of the power of discharging their duties, in which important and frequently intricate calculations are required to be gone through. These symptoms of determination of blood to the head are generally accompanied by coldness and languid circulation in the feet and legs, and by a feeble, and frequent, as well as quick and irritable pulse. On examining the air of the room by appropriate instruments, the author notices more especially three circumstances in which it differs from the external air: first, its temperature, which is maintained with great uniformity within a range of 62° to 64°; secondly, its extreme dryness, which, on one occasion, measured by Daniell's hygrometer, was 70 per cent.; and thirdly, its negatively electrical state, as indicated by the condensing gold-leaf electrometer. In all these qualities the air respired by the inmates of the room bears a close resemblance to the pestilential blasts of wind which, having passed rapidly over the scorching deserts of Arabia and Africa, constitute the *Simoom* of those regions, and are well known by their injurious effects on animal and vegetable life. To these noxious qualities is superadded, as in the air of all rooms heated through the medium of cast-iron pipes or stoves, an offensive smell, arising partly from the partial combustion of animal and vegetable matters always floating in the atmosphere of a town, and perhaps also from minute impregnations of carbon, sulphur, phosphorus, or even arsenic, derived from the metal itself. The author expresses his surprise that in the recent report of the Parliamentary Committee on the subject of ventilation, no reference is made to the methods employed for that object in factories, although they afford the best models for imitation, being the results of innumerable experiments made on a magnificent scale, with all the lights of science, and all

the resources of the ablest engineers. He proceeds to describe these methods; and is then led to investigate the comparative efficiency, with a view to ventilation, of a draught of air resulting from a fire and chimney, and that produced by the rotation of a fan-ventilator. He shows that a given quantity of coal employed to impart motion to the latter, by means of a steam-engine, produces a ventilating effect 38 times greater than can be obtained by the consumption of the same fuel in the ordinary mode of chimney ventilation. Accordingly, he strongly advises the adoption of the former in preference to the latter: and inveighs against the stove-doctors of the present day, who, on pretence of economy and convenience, recommend the slow combustion of a large body of coke, by means of a slow circulation of air; under which circumstances, it is well known to chemists that much carbonic oxide, a gas highly pernicious to all who respire it, is generated; accompanied, at the same time, by a comparatively small evolution of heat. In order to obtain the maximum quantity of heat from a given mass of fuel, its combustion, he observes, should be very vivid, and the evolved caloric should be diffused over the largest possible surface of conducting materials; a principle which has been judiciously applied in several French factories. It has been proved that work-people employed in calico-drying rooms, heated according to the plan here reprobated, become wan, emaciated, and diseased; while in rooms in which the air is more highly heated by means of steam-pipes, they preserve their health and florid complexion.

L. & Ed. Philo. Mag.

Note on the Manufacture of Platinum, by M. Pelouze.

The method of Wollaston in the fabrication of platinum is only followed by those who make this metal an article of commerce. Chemists do not prepare malleable platinum for the requirements of their laboratories, and in their public lectures its preparation is never exhibited. M. Liebig is, I believe, the only one who manufactures it during his course. Although the method he follows is precisely that of Wollaston, and therefore presents nothing new in a scientific point of view, yet it may be alike useful and agreeable to chemists to retrace the steps of a process which is too much neglected and yet so easy of execution, that we may say there is no operation whatever more simple or expeditious than that of the manufacture of malleable platinum, made in the simple apparatus described below. It is a hollow cylinder slightly conical, one of the extremities of which is closed by a small, but very thick, metallic plate. After having decomposed, at as low a temperature as possible, the muriate of platinum and ammonia, the *froth* which is produced is separated by a piece of wood; with this and a little water a clear paste is to be made, and introduced into the cylinder. An iron piston is then introduced into the cylinder, and after having pressed it at first very gently for a minute or two, it is then compressed with the greatest possible force. An iron ring, by which the base of the cylinder is supported, being struck with a hammer, affords us facilities for getting at the piece of platinum which is thus formed. The platinum taken from the cylinder has already a high density, and brilliant metallic lustre. It is dried with a gentle heat; and, after having been exposed for a quarter of an hour to a white heat, it is rapidly withdrawn from the crucible, and receives a single blow of the hammer. It is then again exposed in the fire four or five times, and the number of the strokes from the hammer are only gradually increased. In less than half an hour the whole operation is finished; and it is so easy that the result is always certain. I now exhibit

to the Academy a spatula and the blade of a knife of platinum, which I myself saw prepared in a few minutes at Giesen in M. Liebig's laboratory.

Edin. New Philos. Jour.

London Caoutchouc Company. Mr. Sievier's Patents.

The rapid increase in the use of India-rubber, or caoutchouc, as a material for the manufacture of various articles, has led to a speculation, having for its object the introduction of Mr. Sievier's patents to the public, under the title of the "London Caoutchouc Company." It has apparently met with a favourable reception, as the shares have been quoted in the market at a premium. The first patent, dated 1st December, 1831, and the second patent dated January 17th, 1833, are to secure the right of manufacturing cables, ropes, whale fishing and other lines, lathe and rigging bands, bags and purses; and for the manufacture of elastic goods or fabrics, very much in the mode so well distinguished by F. Peale, Esq., at page 109 of our last volume.

The third patent is dated February 7, 1836, and is for "An improvement in the means of dissolving and preparing caoutchouc, or India-rubber, for various purposes." This patent is solely for the application of Ammonia to the dissolving of India-rubber. The following is the process which the patentee describes:—

"I take caoutchouc, or India-rubber, cut into small pieces, and put them into any convenient vessel that may be closed at the mouth; I then fill the vessel with liquor ammonæ, so as to entirely cover the India-rubber; in a few months it will be dissolved, or its particles separated; I then put the solution so made into a still or large retort, and by the application of heat, nearly the whole of the ammonia will be distilled over in a gaseous form, and is to be taken up in the usual way by cold water; in that state it again becomes liquor ammonia; in this operation of distillation I prefer using a water-bath, as the India-rubber by that means cannot be subjected to a heat of more than 212° of Fahrenheit; the ammonia assumes a gaseous form at 130°. On separating the ammonia by distillation as above, the India-rubber is left held up in the water, and in that state may be applied for water-proofing cloths, or making solid masses of any form; by subjecting the solution to evaporation, any degree of consistency may be given to this solution by increasing or diminishing the quantity of water mixed with the previously dissolved India-rubber. * * * I claim as my discovery or invention the use or application of ammonia to effect the solution or separation in the manner herein described."

Mec. Mag. G.

Fabrication of Beet Sugar.

From the *Receuil Industriel*, for November 1836, we make the following abstract in relation to the expenses and profits of the cultivation of beets and the manufacturing of sugar in France.

We shall take as an example the factories of our northern departments, where this enterprise has been longer and most profitably pursued. In this part of France a hectare of land suitable for the cultivation of beets, rents at a medium price for 70 francs a year. The occupier has to pay a duty of 12 francs per hectare, so that the soil of each hectare costs him 82 francs.

The expenses of seeding and culture including manure, labouring, weeding, and gathering the plants, may be estimated at 300 francs; hence a hectare of beets costs in these departments about 382 francs. The produce

from this quantity of ground is from sixty to eighty thousand weight of roots. Taking 76 as a medium we find that the manufacturer who is also the cultivator of his beets, pays for them about 5 francs 45 centimes per thousand. Although in some favourable circumstances from 8 to $8\frac{1}{2}$ per cent. of sugar has been obtained from the beet, it would not do to estimate the ordinary product at more than 5 per cent. At this rate the thousand of beets would yield 50 demi kilog. of sugar, and consequently the produce of a hectare would be 3,500 demi kilog. of sugar.

The cost of fabrication, including animal charcoal, fuel, manual labour, wear and tear of machinery, office duties, &c. amounts at least to 750 francs to bring to a crystaline state the sugar of a hectare of land. Hence the 3,500 lbs. of sugar which the hectare produces comes, altogether to 1132 francs, which makes a pound of sugar cost 32 centimes; but from this we may deduct the value of the molasses (about two per cent. of the weight of the beets) and of the residue of the pressed roots, (fifteen per cent. of the whole) which may be reckoned at eight centimes—so that the pound of sugar stands the manufacturer at the cost of twenty-four centimes.

The mean value of a hundred weight of sugar is 55 francs, and if these were the only considerations in the cost and produce, the profits would be enormous; but we must take into account the capital necessary to be embarked in the operation. Now it is estimated that a factory adapted to the working of five millions of pounds of beets, would require in the cost of land, buildings, and machinery, an outlay of 150,000 francs. The interest of this sum at 10 per cent. divided by the 250,000 pounds of sugar obtainable from the above quantity of roots, would come to 6 centimes per pound, which extends the cost to 30 centimes or 30 francs the hundred. This would yield a profit on five million of roots, of 60,000 francs a year; but such a profit requires an extensive fabrication. The advantages to those who operate on a small scale are much less in proportion. Nevertheless there have been, of late years, in the northern departments, small manufacturers, who perform the labour within themselves and do it with advantage. We may cite among those who work in a small way, a John Joseph Lecerp who keeps a small inn at Onain near Valenciennes. By means of a very simple apparatus he succeeds in making in one season about a thousand pounds of sugar. A single room fourteen feet square, contains his clarifying and evaporating vessels. His wife's lye-tub serves for a refrigerator. The only decent instrument which he possesses is a rasp which makes 400 revolutions in a minute and which is worked by his children. His sugar is of good quality, and he has even obtained the medal of the Agricultural Society, by way of encouragement.

The imperfect means which these small fabricators employ, only allows them to obtain 3 or at most 4 per cent. of sugar from their roots, and they find this advantageous only by the exemption which they enjoy from taxation. Even the show of an attempt in the Chamber of Deputies last season to extend the tariff, induced some of them to give up, and among those was Joseph Lecerp. Should such a law be passed, only rich manufacturers and very large establishments can be successful, and in this state of things it is next to a certainty that foreigners would appropriate to themselves a fabrication which they have studied among us, and would eventually supply us with the sugar of their production which would more than rival our indigenous manufactory. G.

Purification of Tar.

Tar contains a certain quantity of pyroligneous acid from the wood which produces it, and which in time occasions the destruction of cordage by thickening, in the manner of pitch, as soon as the oily portions have disappeared. This change prevents the threads from stretching and of course they must break, although they may still be sound. The acid should therefore be carefully extracted from the tar. This is effected by washing it in clean lime water. They are to be well agitated together in any suitable reservoir, by a convenient instrument of any kind, changing the lime water frequently, and continuing the operation until a good test paper indicates a freedom from all acidity.

Tar thus purified is a good preservative of wood, which it defends from decay much longer than common tar, because the acid attracts moisture from the air and retains it in the fibres of the wood; but purified tar repels moisture and thus acts as a preservative. G.

Jour. des Conn. Usuelles.

Acetiferous Lamp.

A very concentrated vinegar may be produced by the process of Dobereiner. Take a small glass vial and fill it with absolute alcohol. Introduce into its neck a small glass funnel, into which press some spongy platina and a thread of cotton, which dipping into the alcohol draws it to the platina. The latter acting on the air, the oxygen is absorbed and the alcohol converted into very strong vinegar, which may be collected by placing over the vial a bell glass resting on a glass dish. Dobereiner recommends the following method for preparing a very energetic platina. Make a kind of paste with ammoniacal muriate of platina and ammonia: put it into a platina crucible well covered and expose it to the action of fire. Ibid.

Progress of Physical Science.

On the Causes of the Tornado, or Water Spout. By R. Hare, M. D., &c.

In July 1835, I visited the scene of the tornado, which had in the previous month produced so much damage in and near New Brunswick, New Jersey, and heard it described by various witnesses, and have likewise been edified by the observations made respecting its effects by professors Henry, Torrey, Johnson and other sagacious and learned observers, and especially those of my friends, professor A. D. Bache, and Mr. Espy. Probably in no other instance have the effects of a tornado been so faithfully and skilfully traced, ascertained and registered. Professor Bache regularly surveyed the path of the devastating agent, and ascertained the bearings of the various bodies prostrated by it, so as to make several accurate plots. From an examination of these, the proximate causes of the changes effected, are those of a vertical current at the centre, or axis, of the tornado, and of a horizontal conflux of the air towards that axis from the surrounding space. Some trees appear to have been thrown down on the approach of the hiatus, both directly in front of it and on either side; some fell at right angles, others obliquely to the path. Hence they were found to have a great vari-

ety of bearings, but always pointing towards the path. The time of their falling, and consequently the direction agreeably to the observations of professor Bache, appear to have been determined not only by the extent of the force to which they were exposed, but likewise by the strength of their roots, or the degree of protection afforded them by other bodies, trees or houses for instance. On these accounts, neighbouring trees, falling at different times, had different bearings; but that they all fell towards the point occupied by the axis of the tornado at the time of their overthrow, appears to be consistent with the facts. In one instance, both professor Bache and Mr. Espy observed that the post of a frame building, being dislodged from the stone on which it rested, was first moved towards the path of the tornado in one direction about eighteen inches, marking its course by a furrow in the ground, and afterwards moved in another direction, nearly at right angles to the former, leaving a similar indication of the course in which it had moved. Intermediately between the time when the tornado bore in those directions, the frame was protected by a house.

While the phenomena above described sufficiently indicate the existence of a horizontal conflux of the air, that of a vertical force was demonstrated by the transportation of the debris of the houses and trees, as well as lighter bodies, to a great distance. A lady's reticle was carried seven miles from New Brunswick, and a letter twenty miles. The piece of timber, technically called the plate, on which the rafters of the roof of a church in New Brunswick rested, was carried nearly a quarter of a mile, and lodged in some trees beyond the Raritan. The fields, on the other side of that river, were strewed with shingles torn from the houses in the town.

After maturely considering all the facts, I am led to suggest that a tornado is the effect of an electrified current of air, superseding the more usual means of discharge between the earth and clouds in those sparks or flashes which are called lightning. I conceive that the inevitable effect of such a current would be to counteract within its sphere the pressure of the atmosphere, and thus enable this fluid, in obedience to its elasticity, to rush into the rarer medium above.

It will, I believe, be admitted, that whenever there is sufficient electricity generated to afford a succession of sparks, the quantity must be sufficient, under favourable circumstances, to be productive of an electrical current; and that light bodies, lying upon one of the electrified surfaces, may be attracted more or less by the other.

The phenomena of the rise and fall of electrified pith balls, called electrical hail, sufficiently justify this last mentioned statement; while the continuous stream is illustrated by the electrical brush, or the blast of air produced by a highly electrified point.

It will also be conceded, that thunder and lightning are caused by discharges of electricity between the earth and clouds, analogous to those of a Leyden jar or coated pane; the air performing the part of an electric in place of the glass, while the cloud acts as a coating.

It follows that the phenomena above mentioned as liable to arise between oppositely electrified bodies, may be expected to take place between the clouds and the earth, with effects as much exceeding those produced by human agency, as the snap and spark of an electric battery are exceeded by thunder and lightning. If in the one case pith balls and other light bodies are lifted; in the other, water, trees, houses, haystacks and barns may be

powerfully affected.* If from a point electrified by a human contrivance, a blast of air may be induced; it is assuredly not unreasonable to ascribe to the analogous electrical apparatus of nature, aided by the elasticity of the air, a vertical hurricane. It was under the well founded impression that lightning may be superseded by a current, that we have been instructed by Franklin, to surmount our lightning rods by metallic points, by which electrical discharges from thunder clouds are expected to be conveyed to the earth gradually, which might otherwise pass in sparks of lightning of a formidable magnitude.

If, then, it be demonstrated that a continuous discharge of electricity may become the substitute for lightning, and that within the sphere of the discharge the air may be so lifted as to counteract its gravity; it is in the next place only necessary to advert to facts perfectly well known, in order to point out a cause of acceleration sufficient to account for the well known violence of the tornado.

At the height of fifteen miles, the air has been ascertained to have less than one-thirtieth of the density of the stratum next the earth. Of course this substratum would exercise a force nearly equal to the atmospheric pressure, or about fourteen and a half pounds to the square inch, in order to attain the space occupied by the rare medium, to which allusion has been made. It follows that if the weight of the superincumbent air were removed or counteracted, that the inferior stratum would rise with explosive violence.

While the air is thus carried upwards by the concurrent influence of electrical attraction, and the reaction of its own previously constrained elasticity; other bodies are lifted, both by electrical attraction, and the blast of air to which it gives rise. Hence houses within the sphere of the excitement are burst by the expansion of the air which they contain,

* This figure affords an illustration in miniature of the rise and fall of bodies situated between oppositely electrified surfaces, which, in the gigantic operations of nature, are conceived to be the exciting cause of the tornado. The phenomena represented by it are designated in Pixii's catalogue as "grêle electrique," and may be thus explained. A metallic rod supports one ball within the bell glass, another without, so as to be in contact with the knob of another rod R, proceeding from the conductor of an electrical machine in operation. The brass ball being by these means intensely electrified, attracts some of the pith balls which lie upon the metallic dish in which the bell is situated, and which should communicate with the cushions of the machine. As soon as the pith balls come into contact with the electrified ball, becoming similarly excited, agreeably to the general law they recede from each other and are attracted by the oppositely electrified dish. Reaching the dish, they attain the same electrical state as at first, and are, of course, liable to be attracted again.

their walls being thrown outwards, and their roofs carried away; while, by the afflux of the atmosphere requisite to the restoration of its equilibrium, trees, houses and other bodies are thrown inwards towards the vertical current, from before, as well as from either side.

When once a vertical current is established, and a vortex produced, I conceive that it may continue after the exciting cause may have ceased to act. The effect of a vortex in protecting the space about which it is formed, from the pressure of the fluid in which it has been induced, must be familiar to every observer. In fact, Franklin ascribed the water spout to a whirlwind produced by the concourse of the atmosphere to a given point. His hypothesis was, as I conceive, unsatisfactory, because it did not assign any adequate cause for the concentration of the wind, or for the hiatus which was presumed to be the cause. This deficiency is supplied, if my suggestions be correct.

One fact, of which I am myself a witness, cannot be explained without supposing a gyratory force. About six feet of a brick chimney, without being thrown down, were so twisted on the remaining inferior portion as to be left with its corners projecting.

I have hardly deemed it necessary to advert to the cause of the progressive motion of a tornado, since that would appear evidently due to the current of the atmosphere within which it may be created.

I believe that the electrical excitement which gives rise to atmospheric discharges of electricity, in whatever form they may occur, is usually ascribed to the chemical changes taking place in the atmosphere; especially the formation or condensation of vapour.

Another view of this subject has suggested itself to my mind. It is known that the atmosphere acts generally as an electric, while the earth acts as a conductor of electricity; and since the electric fluid passes through an exhausted receiver with great facility, it results that the rare medium which exists at a great elevation, is equivalent to another conductor. Hence it is evident that there are three enormous concentric spaces, of which that which is intermediate contains an electric, to which the others may act as coatings. When the tendency of the electric fluid to preserve an equilibrium is taken into view, I believe myself justified in the inference, that not only the space occupied by the globe, but the region beyond our atmosphere, or where the air is sufficiently rare to act as a conductor, must abound with electricity. Thus the atmosphere is situated between two oceans of electricity, of which the tension may often be different. Between these electric oceans, the clouds, floating in the non-conducting air, must act as movable, insulated conductors; and from the excitement consequent upon induction, chemical changes, or their proximity to the celestial electric ocean, must be liable to be electrified differently from each other, and from the terrestrial electric ocean.

The phenomena of thunder storms may arise, from the passage of electricity from one electric ocean to the other being facilitated by an intervening accumulation of the clouds, or in consequence of discharges from one insulated congeries of clouds to another through the earth.

The aurora borealis may arise from discharges from one ocean to the other, of electricity which, not being concentrated by its attraction for intervening clouds within air sufficiently dense to act as an electric, assumes the diffuse form which characterizes that phenomenon.

Falling stars may consist of electric matter, in transitu between one portion of the celestial electric ocean and another, tending to restore the

equilibrium when disturbed. They may, in fact, consist of electric matter, passing from one mass of moisture to another; as it may be imagined that in an expanse so vast, in which the tension is so low, there may be a great diversity as respects the quantity of moisture existing in different parts. Indeed, it may be conceived that at times the clouds, insulated from each other, may make their reciprocal discharges through the region occupied by the celestial ocean.

I have been informed by my intelligent friend, Mr. Quinby, who resided for some time in Peru, at an elevation of fifteen thousand feet above the level of the ocean, that the clouds in that elevated region are far more electric than in the lower country of the same latitude; and that, on this account, it was considered as dangerous, at times, to travel in the "*sierras*," or table land. Possibly thunder storms are more frequent in warm weather, in consequence of the greater elevation which the clouds then attain, and their consequent approximation to the celestial ocean of electrity.

Consistently with the hypothesis which I suggested in my essay on the gales of the United States, the enduring rains which accompany those gales are attributed to the contact of an upper, warm, and moist current of air, with a lower current of the same fluid at an inferior temperature, and moving in an opposite direction. It would follow that, on such occasions, the electricity of the upper region would be diffused among the clouds within the upper stratum, without reaching those existing within the lower current. But in such cases neither stratum would be sufficiently insulated and restricted in its extent to transmit the electricity in a concentrated form, or to be liable to the intense excitement necessary to produce a tornado or lightning.

Trans. Am. Philos. Soc.

Facts and observations respecting the Tornado which occurred at New Brunswick, New Jersey, in June 1835, *abstracted from a written statement made by* James P. Espy, M. A. P. S. By R. Hare, M. D., Professor of Chemistry, in the University of Pennsylvania.

The tornado was formed about seven and a half miles west of New Brunswick, and moving at the rate of about twenty-five or thirty miles in an hour, terminated suddenly at Amboy, about seventeen and a half miles from the place of its commencement. It appeared like an inverted cone, of which the base was in the clouds, and the vortex upon the earth. It prostrated or carried off every movable body within its path; which was from two hundred to four hundred yards wide. Trees which were embraced successively within its axis were thrown down in a direction parallel to its path; those on either side always pointing towards some point which had been under its axis. Houses were unroofed, and, in some instances, unfloored; in others, their walls were thrown down outwards, as if burst by an explosion. There are two facts stated by Mr. Espy, and confirmed by professor Bache, which demonstrate fully the existence of an hiatus. In a house which was exposed to the vertical influence of the tornado, a sheet was lifted from a bed, and carried into a fissure made in the southern wall, which subsequently closed and retained it. The same result was observed in the case of a handkerchief, similarly fastened into a fissure in the northern wall. In some instances, frame buildings were lifted entire from their foundations. Joists and rafters were torn from a house and thrown down at the distance from it of about four hundred yards, and in a direction opposite to that in which the trees not lifted from the earth's surface, were prostrated. Of course lighter bodies, such as shingles, hats, books and

papers, and branches and leaves of trees, were carried to much greater distances. There was no general rain, but hail and rain accompanied the fall of the other bodies. The tornado lasted, in any one place, for but a few seconds: the whole of the damage done at a farm having been accomplished, as the farmer stated, while he was passing from the front to the rear of his mansion, so that, by the time that he reached the back door, there was a perfect calm. Meanwhile, his house and barn were unroofed, and all the neighbouring trees thrown down. The noise which accompanied the phenomenon was by every witness described as terrific, being best exemplified by the rumbling of an immense number of heavy carriages. Every object in its path was bespattered with mud on the side towards that from which it advanced. Houses looked as if roughcast, and individuals were so covered with dirt as to be disguised.

Some thunder and lightning attended the tornado. Some trees, which resisted the onset, yielded subsequently; and hence were piled upon those which had fallen earlier. The weaker trees were undermost, and pointed in the direction in which the tornado approached; while the stronger were on the top, pointing in the direction in which it moved away.

Four different places were noticed, where all the trees lay, with their summits directed to a common centre. In the middle of one of these localities, the house was unroofed, and a handkerchief and sheet were lodged within the fissures in the walls, as already stated. The windows in the same house were all broken, and much of the glass thrown outside. From the evidence, Mr. Espy infers that the apparent height of the tornado was about a mile. He states that there were, on the same day, two other tornadoes about seventeen miles apart; and of which the nearest was about the same distance from that of New Brunswick. He conceives that the phenomena all concurred to demonstrate an "inward motion from all directions towards the centre of the tornado, and an upward motion in the middle." These statements of Mr. Espy are confirmed by professor Bache.

One fact of some importance has not been mentioned by Mr. Espy, which was observed by persons who were upon the ground during, or soon after, the catastrophe. I allude to the partial withering of the foliage of those small trees or shrubs, which, from their suppleness, were like the reed in the fable, neither uprooted nor overthrown. This unpleasant effect was perceptible when I visited the scene. Each leaf was only partially withered. As it would be inconceivable that mechanical laceration could have thus extended itself equably among the foliage, a surmise may be warranted that the change was effected by the electricity associated with the tornado.

Concluding Remarks by the Author of the Article.

I ought, perhaps, sooner to have acknowledged that I am aware it has often been suggested that water spouts might be caused by electricity; but the conjecture has not, as far as my information goes, been heretofore supported by any satisfactory explanation as to the mode in which such a tremendous power could arise from that source. That I am warranted in this impression, will, I trust, appear evident from the circumstance that two of the most distinguished among the late writers in the department of science to which the subject belongs, seem to admit, or to demonstrate, their inability to afford any explanation. I allude to Pouillet, and Despretz.

In his treatise on meteorology, Pouillet introduces two narratives respecting tornadoes, which were analogous in every essential point to that of

New Brunswick. The existence of an hiatus especially is proved by the allegation that the walls of prostrated houses were thrown down outwards. A labourer was first urged forwards, in the next place lifted, and lastly overthrown.

The learned and ingenious author concludes with these remarks:

"Comment cette puissance, quelquefois si prodigieuse, peut-elle prendre naissance au milieu des airs? C'est une question, il faut le dire, á laquelle la science ne peut faire aucune réponse précise. De toutes les conjectures vagues et hasardées, que l'on peut faire sur l'origine de ce météore, la moins invraisemblable est peut-être celle que le regarde comme un tourbillon d'une excessive intensité. Mais une discussion sur ce point nous semblerait prématurée; il faut multiplier les observations, et constater avec plus de précision toutes les circonstances de ces phénoménes."—*Elemens de Physique Experimentale et de Météorologie, vol. 2, p. 727.*

All the information respecting tornadoes afforded by Despretz is comprised in the following paragraphs, which I quote in his own words.

" *Trombe.* La trombe se montre en mer et sur la terre; tantôt elle semble sortir du sein de la mer, et s'élève jusqu'aux nuages; tantôt elle descend des nuages jusqu'à la terre.

" C'est une colonne d'eau cônique qui tourne sur elle-même avec une grande vitesse; elle a quelquefois jusqu'à plus de deux cents mètres de base. Elle est très-commune entre les tropiques: les navigateurs passent rarement près des côtes de Guinée sans en apercevoir plusieurs.

"Les trombes produisent des effets terribles; elles déracinent les arbres, renversent les faibles habitations, soulèvent les voitures, etc.

"On peut se faire une idée des trombes par les tourbillons de pouissiére qui se forment tout à-coup, en été, sur les routes, et qui tournent sur eux-mêmes avec une grande rapidité."—*Traité Elementaire de Physique, paragraph* 656, *page* 828, *par C. Despretz.*

In Nicholson's Journal, quarto series, London 1797, vol. 1, page 583, there is an interesting account of some tornadoes seen from Nice, illustrated by engravings, by M. Michaud, who appears to consider them as the effect of electricity, and infers that he could produce the phenomenon in miniature by the aid of a machine, as thunder and lightning are by the same means illustrated. This I have found to be erroneous, as far as my experience goes, and from a cause which is, agreebly to my hypothesis, quite evident. I mean the absence of the co-operating influence of the air when emancipated by electric attraction from the confinement arising from its own weight.

The theoretic remarks of Michaud are very brief, and, to me, scarcely intelligible, as he does not inform us in what way he supposes the electric fluid to operate.

I have understood, since I conceived my hypothesis, that Beccaria ascribed water spouts to electricity, but I have not had the advantage of learning by what reasoning he justified his inferences. However, should it appear that I have made, through the want of information, any undue claim to priority, I shall cheerfully do justice to any philosopher whose speculations I may have overlooked. Trans. Amer. Phil. Soc.

On the Decomposition of Carbonate of Lime by Heat. By M. Gay Lussac.

It was long ago asserted that the calcination of calcareous rocks was favoured by the presence of water; and this opinion appears to have been

admitted even by the greater number of those who were habitually employed in the preparation of lime. M. Dermas admits the influence of water as unquestionable and gives this double explanation of the fact, that it (the water) either acts upon the carbonate by forming an ephemeral hydrate, expelling the carbonic acid and taking its place until it is itself expelled by a red heat; or, that the water, being decomposed by the carbon employed as fuel, various gases are produced, one of which is carburetted hydrogen. This, reacting upon the carbonic acid of the carbonate, tends to convert it into carbonic oxide, and thus facilitates its separation from the carbonate of lime. Hence, limestone recently extracted and consequently still moist, must be more easily calcined than stone that is nearly dry. The greater number of limeburners are acquainted with this fact, and therefore they water the stone which has been long taken from the quarry, before they place it in the kilns.*

But the first of these two modes of explanation is inadmissable, since hydrate of lime is decomposed at a heat considerably below that which is essential to the decomposition of carbonate of lime under the influence of the vapour of water.

The second explanation, considering the circumstances of the combustion of lime kilns, does not appear to me to apply to the facts. I shall not, therefore, stop to examine it, but proceed to state the observations, which, if I am not mistaken, will furnish the true explanation of the influence of water in the calcination of calcareous rocks.

Having filled a porcelain tube with fragments of marble, I placed it over a furnace whose temperature was easily regulated. To one end of the tube was adjusted a glass retort containing water, and to the other end a glass tube for conveying the carbonic acid. The heat was first raised to the point necessary for decomposing the marble; and then, by accurately closing the ash-hole it fell to a dark red, and the carbonic acid ceased to be given out. At this juncture the water in the retort was made to boil, then the carbonic acid reappeared in abundance. When the vapour was intercepted the flow of gas instantly stopped and was restored only by readmitting the steam. The circumstances were thus successively varied, and with them the results themselves.

It appears thus demonstrable, that the vapour of water really favours the decomposition of carbonate of lime by heat, and that by its concurrence this decomposition takes place at a temperature inferior to that which is ordinarily required.

The action of the water appears to me purely mechanical. When carbonate of lime, exposed to heat, begins to undergo decomposition, there is formed around it an atmosphere of carbonic acid, which presses on the acid still in combination, so that, in order to become disengaged it must overcome the pressure of this atmosphere. Now this can take place only by a previous elevation of temperature,—or, by removing the carbonic acid and leaving a vacuum,—or, otherwise, by displacing it either by the vapour of water or some other elastic fluid, such as atmospheric air, &c.

This explanation is justified by the following experiment. I brought some carbonate of lime, in a porcelain tube, to a temperature a little inferior to that at which it began to decompose, and then I caused a current of atmospheric air to pass into the tube. The disengagement of carbonic acid immediately recommenced and continued in connexion with the current of

* Dumas. Traité de Chimie, &c., II., 482.

common air,—stopped when it stopped, and was renewed whenever it was readmitted.

It appears then to me, evident, that the influence of watery vapour, in the burning of lime is limited to the production, merely, of a vacuum for the carbonic acid, and thus preventing a pressure upon that which remains associated with the lime. With the presence of vapour a less elevated temperature is necessary to dislodge it; but still we must not overrate the importance of this vapour. The water in calcareous rocks is mechanically interposed between their particles; and, if we except a trifling portion which remains confined in the centre of pieces so large that the heat does not reach and evaporate it, a large portion of the water must evaporate without producing any useful effect, but on the contrary with a loss of fuel, before the stone has acquired the temperature needful to its decomposition.

I am convinced then that the vapour of water favours the calcination of limestone, but I am doubtful of the real advantage which it is supposed to produce, because there is no great difference between the temperature at which carbonate of lime is decomposed alone, and that at which it is effected with the concurrence of water. Besides, if the steam exerts only a mechanical action similar to that of atmospheric air, we see no important advantage it can yield over the aeriform current which incessantly penetrates the calcareous mass subjected to calcination.

The increased facility of the decomposition of limestone by aqueous vapour, or more properly, by a vacuum, ought not to remain an isolated fact. It may be regarded as an established principle, that when decomposition by heat, or by a chemical agent, must necessarily produce one or more gaseous elements, it may in general be favoured by retaining the substance in a vacuum, or by preventing the elastic fluids disengaged from pressing on it. And, reciprocally, that we may retard or entirely prevent decomposition by forming round the body the pressure of an elastic fluid, of the same nature as the one disengaged. It was thus, in the curious experiment of Hall, that carbonate of lime was brought into a state of fusion at a very high temperature, without undergoing decomposition, and the influence of an adequate pressure of carbonic acid.

Ann. de Chimie et de phys. Oct. 1835.

Remarks on the preceding article added by Dr. Hare.

It is due to justice to state that in the preceding observations and references Guy Lusac has been anticipated by Berzelius. This distinguished chemist informs us (Traite, vol. 2d, page 358) that calcareous carbonates are more difficult to decompose by heat in a covered crucible, than in one which is uncovered; but that by the introduction of a jet of aqueous vapour, or any gas of a different kind from that which is to be evolved, the evolution of the carbonic acid is effected with greater rapidity and less heat. In explanation of these facts, he alleges that the particles of the gas are less resisted by heterogenous particles than by its own.

Extract of a Letter from Mrs. Somerville to M. Arago, detailing some Experiments concerning the Transmission of the Chemical Rays of the Solar Spectrum through different Media.

In the account of the meeting of *L'Academie des Sciences* on the 21st of December 1835, (Compte Rendu, p. 508) it is stated that M. Arago, after having repeated what was most essential in those experiments by which M.

Melloni proves that the solar rays, while preserving all their luminous properties may be deprived of their calorific power, remarked, that there was another point of view in which the subject might be investigated. He said it would be important to inquire if the means employed by M. Melloni, or other analogous ones, would not enable us to deprive the solar rays of their chemical properties also; or, in other words, if, of the three properties which light possesses when it reaches us from the sun,—1st, That of illuminating; 2d, That of heating; and, 3d, That of destroying or exciting chemical combination, we could not separate the two latter, and retain its simple illuminating power. This experiment, remarked M. Arago, would probably lead to curious results, and I last week almost yielded to the temptation of undertaking the investigation. But, as possibly M. Melloni may have himself thought of it, though quite silent about it in his memoir, I think I had better not prosecute the subjects till after consulting the learned Italian philosopher.

The motives which I had in 1835, said M. Arago, at the meeting of the Academy on the 17th of October 1836, not to interfere in researches which so directly conducted M. Melloni to these beautiful discoveries, still subsist. I shall, therefore, abstain from stating some results to which I have arrived concerning the absorption, or the interception, of the chemical rays. Every one, however, will understand that the same reserve cannot be imposed upon Mrs. Somerville; and I cannot, therefore withhold the interesting experiments of this illustrious lady, from the Academy, and the public.

In my experiments, she remarks, I employ the chloride of silver, which Mr. Faraday was so kind as to prepare for me, and which, accordingly was perfectly pure and white. It was liquid, and might be spread uniformly over paper. Although this substance is exceedingly sensible to the action of the chemical rays, yet as we have no precise means of appreciating the changes of colour produced by their action, some uncertainty as to the result might remain were we to compare only those tints which differ but little from each other; but the results which I shall furnish on this occasion shall be chosen from among those which were in no degree doubtful.

A piece of glass, of a light pale green colour, which was perfectly transparent, and less than $\frac{1}{20}$th of an inch in thickness, did not permit any of the chemical rays to pass; after exposure for half an hour to a very hot sun, the chloride of silver behind the glass exhibited no change of colour whatever.

I have repeated this experiment with many pieces of green glass, which differed both in their tint and thickness, and I have always found that they were all nearly impervious so far as the chemical rays were concerned, and even after they had been subjected to the solar influence for a much longer period than that above stated. As M. Melloni has already found that glass of this colour arrests the most refrangible calorific rays, by associating his results with mine, we are led to conclude, that glass of this colour has the power of wholly intercepting the most refrangible portion of the solar spectrum.

Laminæ of mica, of a deep green colour, are also nearly impervious to the chemical rays; however, when they are very thin, and the solar action is continued for a very long time, then it appears that they do not completely arrest these rays. I fixed, with a little wax, to a sheet of paper which was covered over with the chloride of silver, a sheet of pale green mica from Vesuvius, the thickness of which was not more than the thirtieth of an inch, and I exposed the whole to the rays of a powerful sun; after a

time the sheet of mica being removed, it was found that that part of the paper which it covered retained all its original whiteness, while the rest was wholly of a deep brown colour.

The same experiment has been tried with fine sheets of white mica. Six sheets of common white mica placed on each other did not intercept the chemical rays; the chloride of silver which they covered, at the end of an hour's exposure to the sun, had become quite brown. The same result was obtained after using a single plate of mica, which, however, was still thicker than all the others put together. This substance does not appear to present any obstacle to the transmission of the calorific rays.

These experiments led me at first to suppose that all green substances possessed this property: but I very soon found that this would be drawing too hasty a conclusion; for, having shortly afterwards tried the experiment with a very large emerald, the green of which was very beautiful, though not very deep, and the thickness of which was at least 0.35 of an inch, I found that it readily transmitted the chemical rays. Thus, the matter which imparts the colour to the green emerald has no action on the chemical rays, whilst that which imparts the same colour to glass and mica has great influence over them.

Rock-salt, as might be expected, possesses in a high degree the faculty of transmitting the chemical rays. Glass, too, coloured violet with manganese, and very deep blue glass, such as is common in finger-glasses, likewise very readily transmit these rays. The alteration in the colour of the chloride of silver very speedily takes place in spite of the interposition of a plate of blue glass of the deepest tint, and nearly a quarter of an inch thick.

Among the various substances which I have tried in these experiments, rock-salt and white glass, as also blue and violet-coloured glasses, are those which afford the maximum of permeability to the chemical rays; whilst the green shades of glass and mica present the minimum. Other bodies possess this property in intermediate degrees, and sometimes vary considerably, though the colour is nearly the same. Thus glass of a deep red colour allows very few of the chemical rays to pass, whilst garnet, of an equally deep colour, allows nearly the whole of them to pass. The white topaz, as well as the blue, the pale blue beryl, the cyanite, the heavy spar, the amethyst, and various other substances, transmit the chemical rays with great facility; whilst the yellow beryl does not, so to speak, transmit them at all, and the brown tourmaline as well as the green, have the property in so slight a degree, that I have failed in my attempts to polarize the rays under these circumstances, though I believe it might not be impossible, if thinner plates were used than I had it in my power to employ. In concluding, I may observe, that I purpose shortly to resume the prosecution of the subject.*

Edin. New Philos. Jour.

On the cause of the remarkable difference between the attractions of a Permanent and of an Electro-Magnet on soft iron at a distance. By the Rev. Wm. Ritchie, L. L. D., F. R. S., Prof. of Nat. Phil. Royal Inst., and in Univ. of London.

Attach a piece of soft iron, such as the lifter of a common horse-shoe magnet to one extremity of a slender delicate balance of light wood, and balance it by weights in a scale pan at the other end. Place a permanent

* Would it not be interesting to ascertain by careful experiments, the relative effects of green and white glass shades upon plants, as practised by gardeners. G.

horse-shoe-magnet below it and ascertain its attractive force by weights, both when in contact and when removed to different distances from the soft iron. Remove the permanent magnet and substitute a very *short* electro-magnet of equal lifting power. Remove it to the same distance as before and the attractive power will diminish very rapidly compared with that of the permanent steel magnet.

2. Instead of the short electro-magnet, substitute a very long one (of two or three feet long for example) and of equal carrying power; remove it to the same distances and ascertain its attractive power, and it will be found that its attraction for the lifter will *not* diminish so rapidly as that of the short one. The longer the electro-magnet becomes, the more does it approach to the character of the permanent magnet in all its properties.

3. Make the electro-magnet of *hard* iron or untempered steel and its power at a distance will be much greater than in the electro-magnet of soft iron.

These facts are accounted for by Dr. Forbes, in the following manner:

The perfect equality of *action* and *reaction* must be found to exist in this case as well as in every other in which *force* of any kind is concerned. The electricity which has been decomposed and arranged in the soft iron in the peculiar manner which constitutes magnetism, cannot decompose and arrange the electricity belonging to the lifter without suffering a corresponding *diminution*, and the more difficult the arrangement in the lifter so much greater will be the diminution of power in the electro-magnet. Again, if the electricity in the electro-magnet be easily arranged by the induction of the voltaic helix, it will be easily forced back to its natural state by the *reaction* of that belonging to the lifter. Hence it follows that when the *inducing* power of the electro-magnet is very great (which it is when the lifter is in contact with its ends) it will possess sufficient power to *vanquish* the *coercitive* force of the lifter, arrange by induction a large portion of the electricity of the lifter, and thus exhibit powerful attraction. When the lifter is removed to a certain distance, one-tenth of an inch for example, the power of the electro-magnet being much *diminished* in consequence of the distance, whilst the difficulty of overcoming the coercitive force of the lifter is *increased*, the effect will be very small compared with the former. For if the inducing power be only *equal* to the coercitive force of the lifter, no attraction whatever will take place; and hence the impossibility of magnetizing a large bar of steel tempered as hard as possible, by means of a small permanent magnet with a soft temper.

Now, if the coercitive force of the electro-magnet be increased, which is done either by employing a long magnet, or using hard iron or untempered steel, such a magnet will suffer *less* diminution by the *reaction* of the lifter in the case of increased difficulty of arrangement in the lifter, than in the case of the short electro-magnet of perfectly soft iron.

In the case of the permanent magnet of tempered steel, the electricity belonging to it was arranged with *difficulty*, and after repeated *touches* of another magnet, and consequently it will easily vanquish the coercitive power of a piece of soft iron, and induce a magnetic state upon it, whilst the peculiar arrangement of its own electricity will remain nearly *unchanged*. Hence its *attractive* powers will diminish *nearly* as the squares of the distances of the soft iron from its poles, or *imaginary* centres of accumulation, a law which cannot exist in the case of the electro-magnet the electricity of which is so easily put in motion round the elementary molecules of the iron by the reaction of the lifter. Lond. & Ed. Philos. Mag.

Progress of Civil Engineering.

On the advantage of using a mixture of Powder and Saw-dust in Blasting.

It has been frequently inquired, whether a mixture of powder with saw-dust is preferable, in blasting, to powder alone; and the replies have been various, according to the influence of habit and prejudice, private interest, or want of perseverance in the necessary experiments. The saving of so expensive a material is, however, an important consideration in most mining operations, and although the increased effect appears unaccountable to many who have been used to avoid most carefully the mixture of any foreign substance with the blasting powder, yet this effect may, I think, be explained, simply by the fact, that the wider diffusion or greater looseness of the powder, in consequence of the mixture, gives rise to a more immediate and general ignition of the whole mass.

When the holes are charged with powder alone, the powder cannot all ignite at the same instant, and, consequently, a part of it is without effect, for if a new piece of board be placed before a hole charged with powder alone, grains of powder, which have not become ignited, will be found after the explosion on the board. In order to contribute in some degree towards the more general adoption of this highly useful method, I may be allowed to present to the mining public the results of my experiments, begun in December, 1818, and continued several months, which fully convinced me that mixed powder is, in every case, applicable and advantageous; and, since that period, I have directed this mixture to be constantly used in these mines. The saving I have found to be, on an average, two-fifths of the former annual consumption. The powder used here is from the Stollberg mills—is proof at thirty to thirty-two degrees, and of a middling grain. Before using, it is dried as highly as possible; the sawdust undergoes a similar drying, but without causing it to lose its natural colour, or approach a state of carbonization: in other respects it is in the same condition as when first brought from the saw mills. As the mixture, when packed in the ordinary powder-bags, does not continue in the same relative and equal position, in consequence of the component materials separating, by reason of the difference in their specific gravity, I make use of well tinned plate-iron powder-flasks, with a wide well-closed aperture, from which the men fill the cartridges by means of an iron or wooden spoon.

According to Mr. Varnhagen, the saving of powder in Brazil is three-fourths, which I think is hardly possible. I have made trials with this proportion, but no effect was produced; and I was therefore obliged to increase the proportion of the powder to two parts to one of sawdust in bulk, although, in many cases, I could only procure a satisfactory result by a composition of three parts of powder with one of sawdust. Not to offend the prejudices of the workmen, I added powdered charcoal to the sawdust, which answers the same purpose as the sawdust, but charcoal from soft wood is preferable to that from hard, since the former is more inflammable, spreads the powder more loosely, and does not explode in a regular tabular form, which would prevent the simultaneous and instant ignition of the grains of the powder. As it was the opinion of several authors that the evolution of gases, occasioned by the ignition of the sawdust, augmented the effect of the powder, I likewise made trials as to the correctness of this

notion, using sawdust from fresh cut, from dry, and from green woods, and even moistening a pound of it with one-twelfth of its volume of clear water, and although the operation of filling and lighting was performed with the utmost possible dispatch, yet I noticed no augmentation in the effect produced. But even supposing this to be really the case, how many difficulties stand in the way of this method, since it would be exceedingly difficult, if not impossible, to fix the exact proportion of moisture. It is by no means an easy matter to induce the workman to act up to instructions, which his confined views prevent him from fully comprehending; and, indeed, the aversion to innovation is so great, that after the lapse of three years, I still find old miners who will rather purchase their own powder for blasting than use the mixture.—*Karsten's Archiv.* vol. v. p. 199. 1822.

Experiments for ascertaining the difference of consumption in using powder alone, or a mixture of equal parts of powder and sawdust, were continued for eight months, in the year 1818, and the results were the following, four months being employed for using each, under similar conditions:—It appears, first, that the total saving of powder was fully one-third; second, that in the four months during which the mixture was used, although five more men on the average were employed daily, yet only 25 cwt. 37 lbs. were required, instead of 30 cwt. 23 lbs., Silesian measure, leaving a balance in favour of the mixture of nearly 5 cwt.; and, thirdly, that in these months more comparative labour was performed.

Similar experiments were also made at Dortmund, in Westphalia, and were attended with a satisfactory result. It was found that six pounds of mixed powder were equal to eight pounds unmixed, per fathom. It was even found by experiments conducted above ground, that a charge of half powder and half sawdust, well mixed, produced the same effect as powder alone in the sandstone. Counter trials were made of charges as above, and others with the half charge of powder alone, from which it appeared, that although each charge contained the same quantity of powder, yet those without the sawdust were almost entirely without effect. The sawdust from beech and poplar had the same power as that of fir; and it appeared that iron and copper filings answer the same purpose, which proves that the favourable effect produced can be ascribed as little to the looseness of the material mixed with the powder, as to a chemical dissolution of the sawdust; nor can the evolution of gases in the mixture be the cause of it.

The most powerful mixture was proved to consist, not of granulated powder, but of simple powder of purified saltpetre, flower of sulphur, and purified charcoal in the same proportion as the French fowling powder, mixed with beech sawdust. Experiments have been successfully made in other mining districts, as Essen-Werden and the Mark; and measures are being adopted in the mining districts of Westphalia to ascertain with accuracy the exact saving which can be effected.—*Zeitschrift der Mineralogie*, 1829, p. 400. Mining Journal.

Birmingham Railway.

Great gloom has been thrown over the share market, by the announcement that this company will want two millions more money than the estimate. Was not this the company that some year or two back were, week after week, and month after month, trumpeting forth that such a contract

had been made which exactly agreed with the engineer's estimate; that a second was finished some few thousands less than the engineer computed, a third still less, &c. &c.? If all this was true then, how comes it about now, that the whole capital, instead of being quite enough, or too much, is found to be no less than eighty per cent. too little, that is little more than half what it ought to be? We think we have a prospectus in which are these emphatical words, "The cost will be 2,500,000*l.*, and the sufficiency of this estimate has been ascertained by the Committee of the House of Commons, upon the testimony of Mr. Stephenson, supported by two other eminent engineers." What answer can be given to this?

But to be serious, the allowance for contingencies was in this estimate, no less than 294,648*l.* Thus, then, stands the account—

Engineer's estimate . .	£2,205,352
Do's error . .	2,294,648
Total cost	£4,500,000

But it may be 5,000,000*l.*, making the trifling error of two and three quarter millions on two and a quarter!!!

Would it not now be better to abandon this line, with its nine tunnels, and take up Sir John Rennie's, which has only one, and which would cost about one-fourth of the amount, or little more than the second additional sum now wanted. If, as stated in the report, they have spent only 2,280,000*l.*, and have had in cash unpaid shares, and money already borrowed 3,400,000*l.* they will have about enough to make, as we understand, Sir John Rennie's new line, without borrowing this other million. The tunnels might be converted into dark holes for the punishment of culprits, or would make very excellent gipsy retreats, cow-sheds, pig-sties, &c. &c. We recommend the directors and shareholders to take this into their serious consideration, and they will, no doubt, make us a handsome compliment for the suggestion.

Is it true, as whispered about, that a great portion of this new sum wanted has been spent in buying off opposition, &c.?

Why, this line will cost upwards of 40,000*l.* a mile! or more than double the average cost. Surely this is a second edition of the Liverpool and Manchester Railway, which, if we recollect right, Mr. Stephenson undertook to make for about 500,000*l.*, but which, in reality has, we believe, cost near three times that amount! For the shareholders' sakes, and for the sake of burying in oblivion Mr. Stephenson's sad, sad blunder, we hope the success of the one may equal that of the other.—*Railway Magazine.*

Ibid.

Mechanics' Register.

Wood Joists in English Railways.

The substitution of the continuous wood-joist, bedded in concrete, to support the rail, instead of iron chairs and stone bases at intervals only, is about to take place for the first time on an extensive scale in England. The directors of the Great Western Railway, have recently made their first contract for 13,000 loads of timber for this purpose, the whole of which is probably laid down by this time.—*Corr.*

Mag. Pop. Sci.

Soda Bread.

A correspondent of the *Newry Telegraph* gives the following receipt for making "soda bread," stating that "there is no bread to be had equal to it for invigorating the body, promoting digestion, strengthening the stomach, and improving the state of the bowels." He says, "put a pound and a half of good wheaten meal into a large bowl, mix with it two teaspoonfuls of finely-powdered salt, then take a large teaspoonful of super-carbonate of soda, dissolve it in half a teacupful of cold water, and add it to the meal; rub up all intimately together, then pour into the bowl as much very sour buttermilk as will make the whole into soft dough (it should be as soft as could possibly be handled, and the softer the better,) form it into a cake of about an inch thickness, and put it into a flat Dutch oven or frying-pan, with some metallic cover, such as an oven-lid or griddle, apply a moderate heat underneath for twenty minutes, then lay some clear live coals upon the lid, and keep it so for half an hour longer (the under heat being allowed to fall off gradually for the last fifteen minutes,) taking off the cover occasionally to see that it does not burn. This, he concludes, when somewhat cooled and moderately buttered, is as wholesome as ever entered man's stomach. Wm. Clacker, Esq., of Gosford, has ordered a sample of the bread to be prepared, and a quantity of the meal to be kept for sale at the Markethill Temperance Soup and Coffee Rooms. Farm. Mag.

Amber.

The beautiful amber which is found on the eastern shores of England, and on the coasts of Prussia and Sicily, and which is supposed to be fossil resin, is derived from beds of lignite in the tertiary strata. Fragments of fossil gum were found in digging the tunnel through the London clay at Highgate, near London. Mining Journal.

Proportion of Crime to Population.

According to Colonel Forsell, who has recently published a valuable work on the statistics of Sweden, the offenders against the laws in different countries bear the following proportions to the entire population:—

In England as one to	740
Wales,	2,320
Ireland,	490
Scotland,	1,130
Denmark,	1,700
Sweden,	1,500
New South Wales,	22
The United States,	3,500

Farm. Mag.

To make Nectar. (Vin des Dieux.)

Slice very thinly, a quantity of apples, and an equal number of lemons. Cover the bottom of a porcelain dish with a layer of the apples, then a layer of lemon, then a third of powdered sugar and so on till the vessel is full enough. Pour in wine until the upper layer of lemons is well overflowed, put on a cover and let it infuse five or six hours. Press out the liquor through a cloth and filter it. Jour. des Conn. Us.

LUNAR OCCULTATIONS FOR PHILADELPHIA, AUGUST 1837.						Angles reckoned to the right or westward round the circle, as seen in an inverting telescope. ☞ For direct vision add 180° ☜	
Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
5	8	8		N. ap. ☽ and ν Virginis ☽ S 1.'3			
10	10	17	Im.	L Scorpii	1,	16	52
10	10	48	Em.			326	366
14	11	11	Im.	m Capricorni	6,	67	67
14	12	1	Em.			344	354
16	12	13	Im.	K Aquarii,	6,	195	183
16	12	39	Em.			235	229

Meteorological Observations for April, 1837.

Moon.	Days.	Therm. Sun rise.	Therm. 2 P.M.	Barometer. Sun rise.	Barometer. 2 P.M.	Wind. Direction.	Wind. Force.	Water fallen in rain.	State of the weather, and Remarks.
				Inches	Inches			Inches.	
	1	40°	44°	29.40	29.56	W.	Blustering		Cloudy—flying clouds.
	2	32	44	85	86	W.	do.		Clear—clear.
	3	38	56	55	55	SW.	Moderate.		Clear—clear.
	4	34	33	75	75	NW.	do.	.4	Cloudy—snow—cloudy.
⊗	5	32	42	70	66	NE.	do.		Cloudy—do.
	6	32	55	80	85	W.	do.		Clear—do.—aurora borealis.
	7	36	55	76	65	SE.	do.		Lightly cloudy—do. do.
	8	52	44	16	10	W.	Brisk.	.60	Rain—thunder shower.
	9	44	46	35	35	W.	do.		Cloudy—flying clouds.
	10	33	34	50	53	W.	do.		Flying clouds—clear.
	11	39	60	60	65	W.	Moderate.		Clear—do.
☾	12	40	64	30.00	95	W.	do.		Clear—do.
	13	46	65	29.80	80	W.	do.		Partially cloudy—do. do.
	14	46	58	40	40	W.	do.		Cloudy—flying clouds.
	15	40	60	70	7[illegible]	W.	do.		Clear—clear.
	16	46	56	44	40	NE.	do.		Cloudy—do.
	17	42	56	56	5[illegible]	W.	Brisk.		Partially cloudy—do. do.
	18	36	58	70	70	NW.	do.	.10	Rain—cloudy.
	19	39	79	73	73	W.	do.		Partially cloudy—do. do.
☉	20	37	60	80	80	W.	do.		Partially cloudy—do. do.
	21	39	54	75	80	W.	do.		Clear—do.
	22	35	50	80	95	W.	do.		Clear—flying clouds.
	23	37	41	90	85	NE.	Moderate.		Cloudy—rain—snow.
	24	33	43	80	80	NW.	do.	1.00	Snow—flying clouds.
	25	34	52	80	75	W.	Brisk.		Clear—cloudy
	26	35	56	75	75	W.	Moderate		Clear—partially cloudy.
	27	40	60	70	75	W.	do.		Lightly cloudy—hazy.
☽	28	42	68	30.10	30.15	W.	Brisk.		Clear—hazy.
	29	42	73	41	7	SW.	do.		Hazy—do.
	30	52	76	29.85	29.80	SW.	do.		Hazy—do.
	Mean	39.10	53.73	29.72	29.70			1.74	

	Thermometer.		Barometer.	
Maximum height during the month	76. on 30th.	. .	30.41 on	29th.
Minimum do. .	32. on 2d 5th 6th.	. .	29.10 on	8th.
Mean do. .	46.41.	. . .	29..71	

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MECHANICS' REGISTER.
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Mechanical and Physical Science,
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AND THE RECORDING OF
AMERICAN AND OTHER PATENTED INVENTIONS.

AUGUST, 1837.

Practical and Theoretical Mechanics.

Report of the Committee of the Franklin Institute of the State of Pennsylvania on the Explosions of Steam Boilers, of Experiments made at the request of the Treasury Department of the United States. PART II. *Containing the report of the sub-committee to whom was referred the examination of the strength of the materials employed in the construction of Steam Boilers.*

[CONTINUED FROM PAGE 31.]

Second method of observing elasticities.]

Another method of approximately determining the elasticity of iron as indicated when subjected to different strains, was to measure directly on the specimen under trial the distance between two points, taken as remote from each other as possible, both when under strain and when that strain was removed.

Thus bar No. 49 having been permanently elongated $\frac{3}{10}$ of an inch, in $20\frac{3}{10}$, under a weight of 273 pounds in the scale, gave a recoil of .05 of an inch. Afterwards with a weight of 301 lbs., and when a permanent elongation of .58 inch, in the same original length had taken place, the recoil amounted to $\frac{1}{348}$ of the total length. After that trial 15 pounds in addition were required to break the bar.

On bar 226 the first permanent elongation was found under a weight of 245 pounds. Under a weight of 280 pounds the elongation in 24 inches was .86 inch, and when relieved it was .82, giving a recoil of .04 in 24 inches,$=\frac{1}{600}$. After this last trial 35 pounds additional weight were required to produce the fracture.

On bar 228, we find that the first elongation was taken under a weight of 232 pounds. With 238 pounds it had become .146 inch on a length of 24; but when relieved the recoil was .046 inch, equal to $\frac{1}{523}$ of the length. Twenty-eight pounds were afterwards required to be added to break the bar.

On bar 230 the elongation took place under 196 pounds. With 317 pounds the recoil on a part originally 24 inches long was .05 inch, equal to $\frac{1}{480}$ of the whole original length, and 13 pounds more were required to produce the fracture.

Experiments and remarks on this subject will be found in Tables XXXVII., XL., LV., LIX., &c.

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Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
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	14	46	58	40	40	W.	do.		Cloudy—flying clouds.
	15	40	60	70	7[illegible]	W.	do.		Clear—clear.
	16	46	56	44	40	NE.	do.		Cloudy—do.
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